

# Psychological Review

RICHARD L. SOLOMON, Editor

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## PSYCHOLOGICAL REVIEW

### THE RELATION BETWEEN CATEGORY AND MAGNITUDE SCALES OF LOUDNESS<sup>1</sup>

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AND

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Since the publication of the empirical results of Stevens and Galanter (1957), contemporary psychophysics has had to face the disturbing fact that two classes of psychophysical procedures are unable to produce the same scale of sensory magnitude. These authors have shown that the relation between category rating scales and direct magnitude scales is nonlinear. To construct their category scales, Stevens and Galanter used the average category value that subjects assigned to each stimulus. The magnitude scale for the continuum was constructed by taking the median values of magnitude estimations that subjects made to the stimuli. The nonlinearity between these two scales was shown to be a pervasive feature of prothetic, or intensive, stimulus continua. The source of the nonlinearity was not obvious, but the authors argued that it might be partially attributable to differences

in the ease of discrimination of stimuli at various points along the stimulus continuum. Other psychologists who have agreed with this latter point have suggested that the differential discriminability or confusability of the stimuli be used in conjunction with the category values to generate a scale (Messick & Abelson, 1957). We shall refer to this kind of scale as a "processed category" scale. The point that Messick and Abelson make is that if the category scale values are determined by a (Thurstonian) model that allows unequal stimulus dispersions and category widths, as in data processing by the method of successive intervals (Gulliksen, 1954; Torgerson, 1958), the nonlinearity may disappear.

The present paper investigates this question experimentally. When these scales are compared we find that rather than being reduced, the nonlinearity increases! But happily, this increased nonlinearity permits a simple transformation from the magnitude scale to the processed category scale. The processed category scale is a logarithmic function of the magnitude scale. Thus, although the nonlinearity is still present, the form of the relation is no longer a mystery.

The form of the magnitude estimation function itself receives some buf-

<sup>1</sup> The present research was supported in part by the Office of Naval Research, under Contract NONR-551 (37) with the University of Pennsylvania, and in part by the Educational Testing Service. The authors wish to thank Carl Helm for supervising the successive intervals analysis on the IBM 650 computer and R. P. Abelson, R. D. Luce, and S. S. Stevens for their suggestions and stimulation throughout the course of the research.

feting in these data. In fact, we show again (Scharf & Stevens, 1960) that the power law first conjectured to describe the magnitude estimation data, must be generalized to include an additive constant. Now it turns out that there are two ways that we can add this constant, either to the independent or the dependent variable. The data cannot help us to decide. But one way makes theoretical sense while the other does not. The theoretical basis for the decision is not intrinsic to the magnitude function itself, but rests upon the relation between the processed category scale and the magnitude scale. Thus the role of the additive constant and therefore the form of the magnitude function itself is shown to be constrained by its relation to the processed category scale. The very existence of a relation between these two scales provides the evidence that we need for deciding about the form of one of them. This is a curious and not altogether common scientific event.

In what follows we shall first describe the experiment and the data that are generated. We shall then consider the empirically observed relations between the two scaling procedures and finally we shall turn to the form of the magnitude function itself.

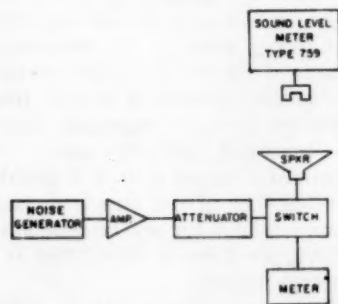


FIG. 1. Block diagram of the apparatus.

## METHOD

### Subjects

The subjects were 71 undergraduate students in the first course in psychology at the University of Pennsylvania. All but two were females. They were between 17 and 22 years old.

### Apparatus

The arrangement of the noise generator, amplifier, attenuator, meter, and loudspeaker that were used are shown in the block diagram in Figure 1. The noises that served as stimuli were monitored during the experiment with a General Radio Sound Level Meter. The meter was at the approximate center of the room. The classroom in which the experiment was performed was an amphitheater with sound reflectant walls. The ambient noise level in the room under experimental conditions was approximately 49 db.  $SPL \pm 3$  db, depending on the reading point. The level of the stimuli varied uniformly over the room. With the stimulus described as 88 db., for example, the level at the back of the room was 86 db. and at the front of the room it was 92 db. This 6-db. variation was constant over the range of the 20 stimuli that were used. The stimuli were controlled by introducing various amounts of attenuation into the generating system. Each of the 20 stimuli was presented five times in an irregular order and on each presentation the stimulus level in decibels re .0002 dynes/cm<sup>2</sup> was recorded. The stimulus variation within these five replications for a fixed point in the room amounted to 2 db. at most and was this great in only 2 of the 100 presentations. The mean stimulus values of the five replications are used to represent the stimuli in all of the analyses.

### Procedure

Upon entering the classroom, subjects were given a form containing a place for their name and 60 spaces for recording their judgments. They were told that this was an experiment to find out how people judged the loudness of noise, and that they were to make 60 judgments using an 11-point scale. They were then given the softest stimulus and told that it should be called "1"; then they heard the loudest stimulus and were told that it should be called "11." They were instructed to assign to each stimulus that number from 1 to 11 that best



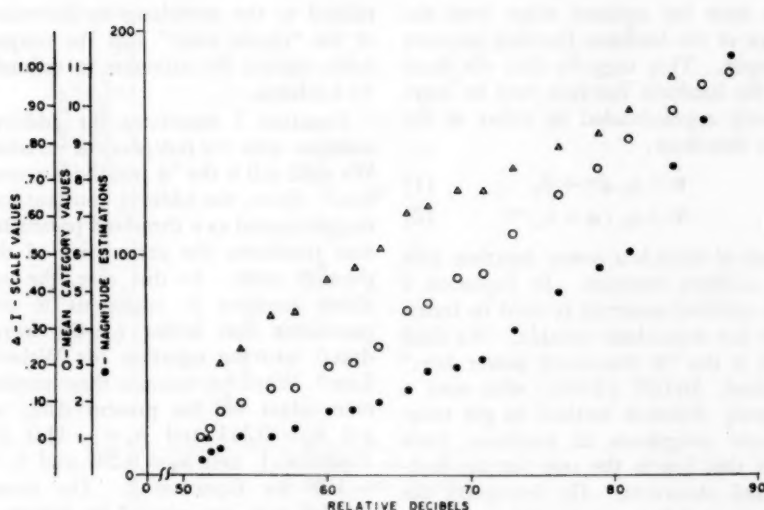


FIG. 2. Relation (filled circles) between subjects' magnitude estimation and relative decibels; mean category values (open circles) and I scale values (open triangles) plotted against relative decibels.

matched the apparent loudness, and to consider each judgment carefully. They were told that their names were recorded so they would take the experiment seriously. The following 20 noise levels (in db.) were presented in an irregular order three times each for 60 trials:

51.4	57.8	67.0	78.8
51.8	60.2	69.0	81.0
52.6	61.8	70.8	84.0
54.0	63.6	73.0	86.2
56.2	65.6	76.0	88.0

After this part of the experiment, and apparently as an afterthought, subjects were asked if they would object to making 40 more judgments in a new and different way. They all agreed. This time they were told that the first noise they would hear should be called "50," and that all the rest of the noises should be given numbers proportional to their loudness. This idea was explained to them in some detail. The experimenter checked on their understanding by giving them problems: for example, "If the second noise seemed three times as loud as the first, Eloise, what number would you give it?" The same 20 stimuli were again presented twice each for 40 trials in a new irregular order. The subjects recorded their judgments alongside the category judgments on

the same recording sheet. They were warned that the new experiment was unrelated to the first, and that they should not be influenced by the first set of numbers.

At the end of the experiment the result sheets were collected and we made a final determination of the ambient noise level.

## RESULTS

### *Magnitude Scale Analysis*

The average magnitude estimations of the 71 subjects for the 20 stimuli are plotted as filled circles in Figure 2. In order to see if the "power law" (Stevens, 1957) for loudness was confirmed, these data are plotted as circles in Figure 3. The straight line through the points is the S scale of loudness (Stevens, 1956), a power function of the form  $\Psi = a_s \phi^{n_1}$  with  $n_1 = 0.3$ . With the exception of the first three weak stimuli the points closely approximate the function. The failure of the weak stimuli to conform to the law has been found in other work (Scharf & Stevens, 1960). When the stimuli

are near the ambient noise level the slope of the loudness function becomes steeper. This suggests that the form of the loudness function may be more closely approximated by either of the two functions:

$$\Psi = a_2 \phi^{n_2} + b_2 \quad [1]$$

$$\Psi = a_3 (\phi + b_3)^{n_3} \quad [2]$$

Each of these is a power function with an additive constant. In Equation 1 the additive constant is used to translate the dependent variable. We shall call it the " $\Psi$  translated power law." Indeed, McGill (1960), who uses a slightly different method to get magnitude judgments of loudness, finds that this law is the rule for the individual observers. He interprets the constant,  $b_2$ , as a personal parameter

related to the stretching or shrinking of the "elastic ruler" that the subject holds against the stimulus to estimate its loudness.

Equation 2 associates the additive constant with the independent variable. We shall call it the " $\phi$  translated power law." Here, the additive constant can be interpreted as a threshold parameter that translates the zero point of the physical scale. In this case, the additive constant is analogous to the parameter that Miller (1947) introduced into the equation for Weber's Law.<sup>2</sup> When we estimate these parameter values for the present data, we get  $n_2 = 0.263$  and  $b_2 = -11.0$  for Equation 1, and  $n_3 = 0.295$  and  $b_3 = -1.28$  for Equation 2. The transformed data are plotted in Figure 3 with the squares fitted by the  $\Psi$  translated power law, and the triangles by the  $\phi$  translated power law. Either of the functions is an improvement over the original equation. The data points can hardly help us to decide which is the more reasonable description. However, an independent reason for choosing the  $\phi$  translated power law as expressed in Equation 2 will be discussed in a later section. Temporarily we shall assume that Equation 2 is appropriate and shall call the scale that it defines the "generalized S scale" or GS scale.

The average category values for the 20 stimuli are plotted in Figure 2 as open circles. When these category scale values are plotted against the GS scale as in Figure 4 (filled circles), the function is concave downward as has been shown in the past (Stevens & Galanter, 1957).

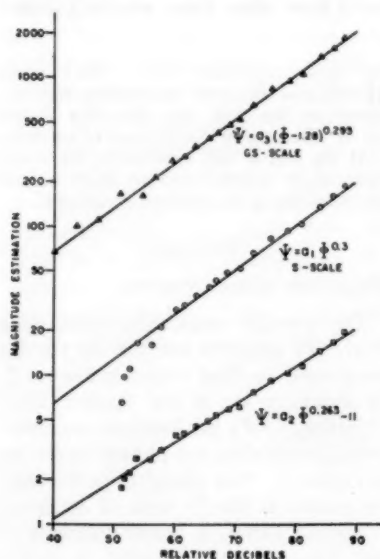


FIG. 3. The three possible magnitude estimation scales plotted against relative decibels. (The magnitude estimation values have been displaced along the ordinate. The circles show the S scale, the squares show the  $\Psi$  translated scale, and the triangles show the  $\phi$  translated scale.)

<sup>2</sup> This form of the power law for the data from magnitude estimation was suggested by Luce (1959, Footnote 3). Stevens (1959, p. 217) used this suggestion to fit data obtained from magnitude estimations of vibratory stimuli.

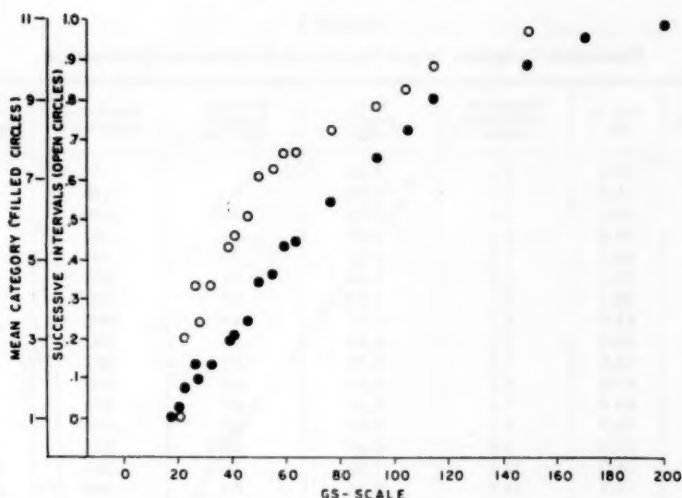


FIG. 4. Customary downward concavity (filled circles) when mean category values are plotted in arithmetic coordinates, and increased downward concavity (open circles) produced by the successive intervals analysis.

#### *Successive Intervals Scale Analysis*

In order to see if a scaling technique that uses judgmental variability in order to obtain a subjective metric would reduce the observed curvilinearity that appears to relate the category scales and the magnitude scales, the category judgments were scaled by Thurstone's method of successive intervals allowing unequal discriminial dispersions (Diederich, Messick, & Tucker, 1957). It has been suggested elsewhere (Messick & Abelson, 1957) that the nonlinearity on prothetic continua may be a result of distortions introduced into the category scale that arises when we ignore variations in subjective category widths and stimulus dispersions. These distortions should be corrected by the method of successive intervals, which allows unequal category widths and unequal discriminial dispersions.

The processed category scale values and discriminial dispersions are given

in Table 1; the category widths beginning with the second category were .23, .16, .10, .05, .05, .05, .06, .07, .16. We see that the category widths and the discriminial dispersions tend to decrease as scale values increase with a slight reversal at the upper extreme. When this processed category scale is plotted against the GS scale (Figure 4, open circles), the curvilinearity is accentuated rather than reduced, indicating that correction for unequal dispersions in category scaling does not account for the divergencies between category and magnitude scales.

However, a transformation to rectify the observed nonlinearity has been suggested for processed category scales of prothetic continua (Helm, Messick, & Tucker, 1959). A rationale for the transformation is based on the following kind of argument: The Thurstone scaling models, of which successive intervals is one, assume that  $p(a, b)$ , the proportion of times  $a$  is judged louder

TABLE 1  
PROCESSED CATEGORY SCALE VALUES AND DISCRIMINAL DISPERSIONS

Stimulus	SPL in db.	Magnitude estimation values	Mean category values	Processed category ( $i_s$ ) scale	Discriminal dispersions	T scale = $e^{at}$
1 <sup>b</sup>	51.4	2.1	1.00			
2	51.8	2.2	1.20	.0	.158	1.00
3	52.6	2.4	1.76	.20	.089	1.82
4	54.0	2.6	1.95	.24	.102	2.05
5	56.2	3.1	2.37	.33	.107	2.69
6	57.8	3.4	2.37	.34	.083	2.77
7	60.2	4.1	2.93	.43	.097	3.63
8	61.8	4.5	3.07	.46	.096	3.97
9	63.6	5.2	3.45	.51	.093	4.62
10	65.6	5.9	4.46	.61	.062	6.23
11	67.0	6.5	4.65	.63	.060	6.62
12	69.0	7.5	5.36	.67	.056	7.46
13	70.8	8.4	5.43	.67	.068	7.46
14	73.0	9.8	6.49	.73	.055	8.94
15	76.0	12.0	7.59	.79	.061	10.70
16	78.8	14.8	8.33	.83	.060	12.06
17	81.0	17.0	9.12	.89	.074	14.44
18	84.0	21.0	9.94	.98	.068	18.92
19 <sup>b</sup>	86.2	24.4	10.63			
20 <sup>b</sup>	88.0	28.0	10.91			

\* Arbitrarily translated so that the smallest scaled stimulus is zero.

<sup>b</sup> Not scaled because of insufficient variation for the successive intervals analysis.

than  $b$ , is a function of differences in subjective scale values. Suppose, however, that  $p(a, b)$  reflects ratios of subjective scale values ( $t_a/t_b$ ) instead of the differences ( $i_a - i_b$ ) that are assumed in the Thurstone model. The application of the Thurstone technique to these proportions introduces an implicit logarithmic transformation: i.e., if the proportions represent subjective ratios, and they are treated as differences in applying the model, the subjects' scale values are implicitly transformed so that  $\log t = i$ . This would imply the necessity for an antilog transformation to return us to the appropriate scale. If the model fits, then processed category scale values are invariant up to a linear transformation (cf. Adams & Messick, 1958), and the appropriate antilog transformation to apply in order to recover the implied scale is:

$$t = e^{at + \beta}$$

where  $t$  is the transformed scale value,  $i$  is the processed category scale value, and  $\alpha$  and  $\beta$  are free constants estimated from the data. The processed category scale values for the present data were transformed (with an approximate estimate of the exponent) by the function  $t = e^{.8i}$  to generate what is here called the "T scale"; the transformed values are given in Table 1. The transformed, processed category values (T scale) are plotted in arithmetic coordinates (Figure 5) against the magnitude values obtained from the GS scale. The plot is essentially linear.

#### *Relation between the T Scale and the GS Scale*

Because the antilog transformation eliminated the nonlinearity between the processed category scale and the magnitude scale, the rationale for the transformation gains some support, although alternative interpretations can be given.

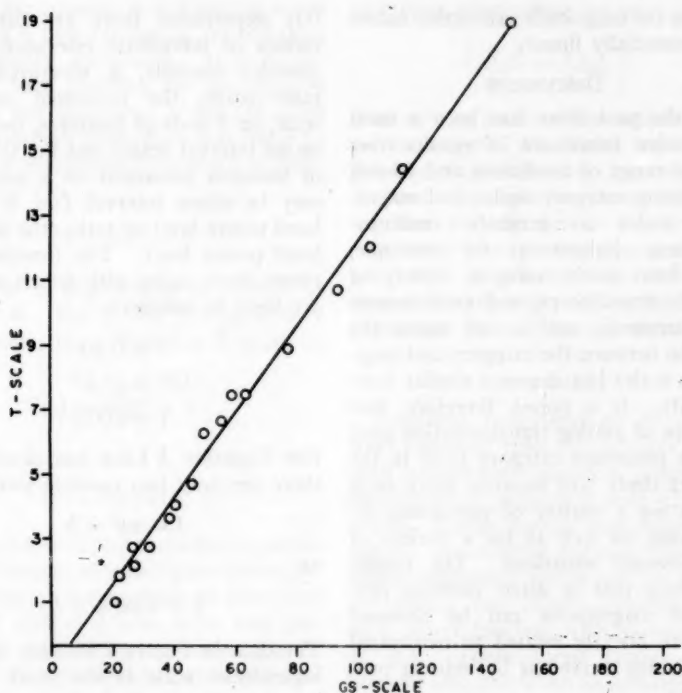


FIG. 5. Relation (open circles) between the T scale values and the GS scale values for the stimuli in arithmetic coordinates.

The result suggests that the processed category scale generated by analyzing the variability among category judgments is such that equal distances on the processed category scale correspond to equal ratios on the scale of subjective magnitude. The result could also suggest that processed category scaling on a prothetic continuum produces equated ratios rather than equated intervals, generating what Stevens (1957) has called a logarithmic interval scale. An alternative, and at present experimentally indistinguishable interpretation, is that category judgments reflect equated intervals on a log scale.

Some additional data, consisting of loudness judgments by 20 subjects of

14 stimuli on a seven-point category scale, were obtained from Stevens for reanalysis by the above procedure. The category values for these data were plotted against the magnitude estimation values in Figure 8b of Stevens and Galanter (1957). Eleven stimuli exhibited sufficient variability to permit analysis. The successive intervals analysis of the category judgments again increased the curvilinearity in relation to the magnitude estimation values, and discriminial dispersions and category widths again tended to decrease as stimulus scale values increased. When an antilog transformation of the form  $t = e^{4t}$  was applied to the processed category scale, the plot of the transformed values



against the magnitude estimation values was essentially linear.

#### DISCUSSION

In the past there has been a most impressive invariance of results over a wide range of conditions and stimuli in relating category scales and magnitude scales on prothetic continua. Loudness judgments, for example, have been made using a variety of stimuli, transducers, and environmental surrounds, and in all cases the relation between the category and magnitude scales has shown a similar non-linearity. It is hoped, therefore, that the type of antilog transformation used on the processed category scale in the present study will linearize these scale values for a variety of perceptual dimensions, as well as for a variety of experimental situations. The results also show that by direct methods perceptual magnitudes can be assessed that are linearly related to perceptual magnitudes arrived at by indirect procedures.

A number of problems still remain to be resolved. What is the appropriate form of the psychophysical law for magnitude judgments—the  $\Psi$  translated power law or the  $\phi$  translated power law? Is the form of the subjective scale for loudness a ratio scale or an interval scale, or sometimes one and sometimes the other? The answers to these questions are not easily obtained from the experimental data. However, certain theoretical results of Luce (1959), in conjunction with these data, do enable some answers to be proposed.

Luce studied the class of permissible functional relations between variables with measures that are invariant up to some specifiable set of transformations. For the present purpose we need only consider his result for variables measured on interval and ratio scales. In

this experiment there are three variables of immediate relevance. The stimulus variable,  $\phi$ , measured on a ratio scale; the processed category scale, or  $I$  scale of loudness, measured on an interval scale; and the GS scale of loudness measured on a scale that may be either interval (the  $\Psi$  translated power law) or ratio (the  $\phi$  translated power law). The functions between these scales with which we deal are three in number:

$$I = f(\phi) \quad [3]$$

$$GS = g(\phi) \quad [4]$$

$$I = h(GS) \quad [5]$$

For Equation 3 Luce has shown that there are only two possible forms:<sup>a</sup>

$$I = a\phi^n + b$$

or

$$I = a \log \phi + b$$

The data in Figure 2 indicate that the logarithmic scale is the most appropriate psychophysical relation. Equation 4 allows us three different functions depending upon whether the GS scale is interval or ratio. If the GS scale is an interval scale, then Equation 4 is satisfied by either:

$$GS = a\phi^n + b$$

or

$$GS = a \log \phi + b$$

of which only the former is acceptable on the basis of the data. But if the GS scale is a ratio scale, then the form of Equation 4 can also be:

$$GS = a(\phi + b)^n$$

<sup>a</sup>The constants and exponents in all of the equations that are displayed below are not distinguished from one equation to another, but it should be noted that in the various equations they take on various values.

where the constant  $b$  is a unique value defining a new ratio scale, rather than an arbitrary translation. It is Equation 5 that provides the leverage to answer our question. If the GS scale is an interval scale, then according to Luce's development Equation 5 is satisfied only by:

$$I = aGS + b$$

which is clearly wrong as shown in Figure 4. But if the GS scale is a ratio scale, then Equation 5 must be either:

$$I = aGS^a + b$$

or

$$I = a \log GS + b$$

Figure 5 shows that the latter logarithmic relation is obtained experimentally. Therefore, it is reasonable to infer that the GS scale is a ratio scale, and specifically that the  $\phi$  translated power law is the appropriate psychophysical relation.

Using this conclusion we turn to a discussion of the interrelations between the two kinds of psychophysical laws. When the data are analysed by successive intervals techniques, the results obtained from category judgments yield an interval scale of the form  $I = a \log (\phi + b) + \beta$ , where the new parameter,  $b$ , is introduced as a threshold parameter analogous to its role in the GS scale. Magnitude estimation methods give a ratio scale of the form  $\Psi = a(\phi + b)^e$ . We can rewrite the interval scale as:

$$\frac{I - \beta}{a} = \log (\Phi + b)$$

and the ratio scale as:

$$\left(\frac{\Psi}{a}\right)^{1/e} = (\Phi + b)$$

and then substituting we get:

$$\frac{I - \beta}{a} = \frac{1}{e} \log \frac{\Psi}{a}$$

And so the form of the relation that we must get if the scales are appropriately characterized is just what we observe. It might be worth mentioning that the logarithmic relation that was found between the processed category and magnitude scales was observed independently of the theoretical arguments that we have outlined here.

#### SUMMARY

Loudness judgments of 20 stimuli were obtained from 71 subjects using both magnitude estimation procedures and category judgments with 11 categories. Except for the first three weak stimuli, the relation between the magnitude estimation values and the physical values was closely approximated by a power function with an exponent of 0.3. These three stimuli were brought into line when an additive constant was added to the power function to form the "generalized power law." As in the past, average category scale values were found to be curvilinearly related to the magnitude values.

A successive intervals analysis of the category judgments generated a scale we call the "processed category" scale. For this metric, the discriminial dispersions and category widths tended to decrease as stimulus scale values increased. When the processed category scale was plotted against magnitude estimation values, the curvilinearity was accentuated rather than reduced, indicating that correction for unequal dispersions in category judgments does not account for the divergencies between the scales.

An antilog transformation was applied to the processed category scale. The transformed scale was found to be

linearly related to the magnitude estimation values.

Theoretical considerations lead to the conclusion that the additive constant that was introduced into the magnitude scale should be added to the independent variable. The appropriate form of the new psychophysical scale, the GS scale, is then  $GS = a(\phi + b)^*$ .

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## CODING AND LEARNING IN SHAPE DISCRIMINATION

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There is presently available a number of theories of discrimination learning. The purpose of this paper is to offer criticisms of such theories, not on the grounds that they are the wrong sort of theory to have about behavior, but on the grounds of vagueness and incompleteness. The theories referred to are those which present models of discrimination learning in probabilistic terms. Such theories are abstract, and abstractness in the sense of generality is usually held to be desirable in a scientific theory; however, abstractness defeats its own purpose if it leads to ambiguity of interpretation on the one hand, or neglect of some of the facts which a theory should explain on the other. (The two dangers are, of course, related.) That both these dangers exist for theories of discrimination learning will be demonstrated, and possible moves for overcoming them suggested. The *Schwerpunkt* of the discussion will be the topic of shape discrimination, because the difficulties for discrimination learning theories are here particularly acute, and because possible solutions to the problems involved have already been proposed.

The study of shape discrimination in animals has not been pursued very vigorously since the publication of Lashley's classic studies on the rat (Lashley, 1938). Lashley established certain general features of visual discrimination in the species, which included some work on shape discrimination as such,<sup>1</sup> and he stated that he was unable to detect any principles of

organization of vision in the rat which differed from principles established for human vision. It must be remembered that Lashley was writing at a time when gestalt interpretations of sensory stimulation were dominant, and he was interested chiefly in "general principles of organization" rather than any particular details of shape discrimination. The findings appeared to be unexciting and offered little prospect of further worthwhile investigation, at least within the general theoretical framework current at the time. It is not surprising, therefore, that interest in discrimination has become centered on the characteristics of the learning process itself during the acquisition of visual discrimination, to the neglect of the logically distinct but equally important question of the processes which underly the actual sorting out or analyzing of the discriminanda. Hence the theories of discrimination learning referred to earlier. Examples of the work in this field are the theories of Bush and Mosteller (1955, Ch. 2), Restle (1955a, 1955b), Shepard (1957, 1958), and Atkinson (1959). Whilst the details, and to some extent the aims, of these theories are dissimilar, they share rather similar assumptions about stimuli and the way they affect the organism. For instance, Bush and Mosteller assume that the stimuli confronting an organism in a choice situation constitute a population which is "sampled" from trial to trial (Estes—1950—makes essentially the same assumption), as does Restle, although the latter talks of "cues" rather than stimuli, and Shepard assumes that: "On any given trial a single stimulus

<sup>1</sup> These studies of shape discrimination are open to criticism (see Dodwell, 1957b).

is presented at random from a set of  $N$  stimuli" (does this suggest that each stimulus is discriminable from the rest of the set?). The ambiguities in the use of the words "stimulus" and "cue" will be taken up later; for the moment it is sufficient to note that the idea of a common pool of more or less discrete stimulus units is assumed in each system. If one can relate the units in some way to response probabilities and state rules for the changes in these probabilities with reinforcement or nonreinforcement, one has the basic materials for constructing a stochastic learning model. Ingenious as such models are, their usefulness in furthering an understanding of discrimination learning is limited by at least two factors: first, it is not always easy to decide between models on the basis of empirical tests (Luce, 1959; Walker, 1957); and secondly, the very general assumptions which are made about stimuli, or cues, require much sharper definition than they are given. Nothing is said about the "classification" or "recognition" of cues, nor the sort of system which performs these functions. Clearly the question of the analysis of stimuli is logically prior to the consideration of S-R connections and associated probabilities, because the learning process itself, conceived in S-R terms, is a "black box" with stimuli as the inputs. The form of the inputs will affect the operations of the black box, and to the extent that one leaves the specification of the inputs vague one reduces the possibility of definitive analysis of the learning operations. The very general assumptions about stimuli made by discrimination learning theorists are inadequate in this sense, as will be demonstrated below.

In some cases questions concerning the type of classification system seem not to present too great a problem, for

instance when human subjects are used who can give verbal evidence of recognizing different stimuli (naming letters used as stimuli, for instance) or when a set of stimuli is ordered on a psychophysical continuum where there is usually a simple ordinal relationship between the stimulus judgments and their associated physical scale. (This does not mean that we know what the effective classification system is, of course, only that we have some evidence for the type of classification being made in terms of an independent variable.) We note, however, that classification of shapes presents a special problem, since no system of defining shape as an independent variable for psychological experiments is available (Arnoult, 1954) and this could well hold for other "stimulus properties" too. The presently available evidence suggests rather strongly that in some animals, including the octopus and the rat, shape is classified in a definite way. Although the types of classification have not been fully identified (Dodwell, 1960, 1961), the fact that such classification does occur is a good reason to suppose that any theory about discrimination learning should take some account of it. Stochastic learning theories do not in general place restrictions on the types of stimuli impinging on the organism and the procedure has been to assume that input classification is not an important factor; no specific assumptions are made about how the incoming stimuli are processed.

#### THEORIES OF DISCRIMINATION LEARNING

The pertinence of these remarks may be illustrated by referring to a particular theory of discrimination learning. Restle (1955b) in constructing his theory makes—among others—the following assumptions: (a) that



in a discrimination learning situation there will be a number of relevant, and a number of irrelevant, cues; (b) that from trial to trial relevant positive cues will become conditioned to the correct response (which may be simply "no response" to a "negative" cue); (c) that irrelevant cues will become "adapted" or neutralized; (d) that the probability of a correct choice can be expressed as a function of the number of cues conditioned and adapted to date; and (e) that the rate of conditioning and adapting is governed by a quantity  $\theta$ , the proportion of relevant cues  $r/(i+r)$ . A neat expression for  $p(n)$ , the probability of a correct choice on trial  $n$ , is deduced in terms of  $\theta$  and  $n$ , and the plot of  $p(n)$  against  $n$  gives an S shaped curve for learning. Similar results are obtained by Bush and Mosteller (1955, Ch. 2) using the algebra of set theory.

No specific assumptions are made by Restle about the nature of the cues, either in terms of their physical properties or their psychological attributes. So one might hold that this theory, and others like it, could be applied in any discrimination learning situation, especially as a method is given for calculating  $\theta$  from  $E$ , the total number of errors in learning the discrimination perfectly. Thus, although the theory does not generate hypotheses about cues, it should be possible to use it to test such hypotheses. However, the very generality of the concept of a cue, as here used, leads one to question the usefulness of this approach to the problems of discrimination (especially of shape), and in fact it turns out that the concept of cue is highly ambiguous. There will, of course, be both relevant and irrelevant aspects in any discrimination situation, but it seems unlikely that a simple partitioning of cues, all, as it were, equipotential, is justifiable.

In Restle's theory it is not clear whether "cue" is being used to refer to aspects of the stimulus display, which would be the normal use, or whether it is being used to refer to the signals, or aspects of them, which reach the cortex from peripheral receptors. If the former, there is a further ambiguity, in that the cues could be ordinary perceptual properties of the display, such as orientation, size, color, brightness, etc. (a further problem here is that we have no means of deciding, a priori, which of these cues—if any—are also identifiable for infra-human subjects), or they could be physically identifiable properties, such as rate of change of luminous flux over the display, wave length of light reflected, etc.<sup>2</sup> One assumes that in a discrimination learning situation it is the perceptual properties, or cues, that are important, and this already implies some sort of classifying or analyzing system within the organism. Not to consider what this system might be, and to talk of cues as if they were identifiable or specifiable in some other way, is misleading. In a recent application of this theory (Bourne & Restle, 1959) "cue" seems to be identified, for human discrimination, in the first sense. If cues are ordinary perceptual cues, then in many discrimination situations there may be only one relevant cue (size, for instance) and the number of irrelevant cues is always unspecified. Irrelevant cues are not all in the modality under investigation in a real learning situation (this is especially true for animals, which cannot benefit from instructions); however elaborate one's controls, there are always proprioceptive cues present. In this case  $r/(i+r)$  ( $=\theta$ ), where  $r=1$ , must be small, and hence according to

<sup>2</sup> The relationship between the two is not necessarily simple (cf., for instance, Gibson, 1950; Koffka, 1935; Michotte, 1946).

Restle's theory learning would be negligible. If one restricts "cue" to the modality or modalities under investigation the theory becomes unrealistic, for reasons just given; other cues are always present, and nearly always enter into the process of discrimination learning as a complicating factor. Moreover, there is no good reason to suppose that the cues arising from other modalities are constant over the learning period. In any case the word "cue" used in the everyday sense is too vague to be given currency in a rigorous theory, without proper definition.

The other possibility is that "cue" may be used to refer to properties of signals relayed to the cortex; in this case the same objections still seem to apply. Such cues must be classified in some way; whilst it might be possible to devise a form of coding which would parcel out the available information into "equal units" (cues, in Restle's terms), such a venture would be wide of the mark as far as shape discrimination is concerned. The available evidence (Dodwell, 1957h, 1960, 1961; Krechevsky, 1938; Lashley, 1938; Sutherland, 1957, 1960) indicates that different elements of the visual display in a shape discrimination situation differ widely in their efficacy as discriminanda. Modifications have been proposed (Estes & Burke, 1953) to allow for some such differences, but in rather general terms which would not yield specific predictions about shape discrimination. This discussion of the use of "cues" and "stimulus" in theories of discrimination learning is not original, of course; its inclusion, however, serves to demonstrate the importance of defining adequately the terms employed in constructing a theory. This is particularly important where the theory is an abstract one. It is suggested that an adequate defini-

tion of cue or stimulus should include a concept of stimulus classification to explain known peculiarities of shape discrimination, and allow one to make predictions. That is, a form of coding which would explain the dominance of one sort of cue over another, why some discriminations are possible and not others, why certain transfers occur when a discrimination has been learnt, and not others. Such a system might then reasonably be "connected up" to a stochastic model concerned with the generation of response probabilities. To put the matter another way: the stochastic theories consider the stimulus display in terms of discrete units each of which (initially) yields information about the display. It is suggested that a better procedure would be to consider information reduction (classification) as an additional stage in discrimination learning. It can be argued that the stochastic theories actually generate classifications during the learning of a discrimination, but the point to be made is that empirical studies indicate the presence, at least in adult organisms, of a classificatory system which to a great extent affects the learning of any new discrimination. The stochastic models in effect postulate a *tabula rasa*, and this is a naive assumption to make. Whether the pre-existing classificatory system is itself learned or not is, naturally, open to experimental investigation, although conclusive evidence would perhaps be difficult to obtain.

#### EXPERIMENTAL EVIDENCE

That this criticism is not idle may be demonstrated by referring to two peculiarities of visual shape discrimination discovered by Lashley and Krechevsky, respectively. The former (Lashley, 1938) describes experiments in which rats were taught two different

discriminations simultaneously, viz., horizontal + vs. vertical - striations, and cross + vs. circle -, and were then presented with cross on vertical striations and circle on horizontal striations, that is, positive shape on negative ground, and vice versa. Scores for correct jumps agreed with chance expectancy. This one might try to explain in terms of "approach-avoidance" conflict induced by a mixture of positive and negative cues in each complex shape; however, this sort of explanation will not cover the case where positive shape was shown on positive ground (cross on horizontal striations) and negative shape on negative ground. In this case the scores for correct jumps were slightly better than when the positive and negative cues were superimposed, but still far from perfect (12.5/20 average for four rats). Any theory which considers positive and negative cues as simple algebraically additive units cannot explain this result. It can be explained, however, in terms of the inability of a shape analyzer to "sort out" the signals for two shapes when these are generated simultaneously, for in this case the characteristic features of either signal may be masked by superposition of a second signal, so that neither can be "recognized." Lashley obtained the same results in a second experiment of the same design, using different shapes, so one can be reasonably sure that the results are not simple artifacts of the particular shapes used.

Krechevsky (1938) demonstrated that under certain conditions rats will *not* prefer the shape to which they were trained to jump over a new shape. He trained two groups of rats as follows: Group I learned to discriminate interrupted horizontal vs. interrupted vertical striations (half the group horizontal-positive, the other half vertical-positive); Group II were trained to

discriminate the same horizontal or vertical patterns from a triangular pattern of small squares. On subsequent test discriminations the horizontal (or vertical) patterns which were positive during training were paired with horizontal (or vertical) solid lines. For Group I the solid lines were *perferred to the original positive figures*. Krechevsky (1938) interpreted this finding along gestalt lines, viz.:

forces (of attraction among the squares due to proximity) thus generated are of such a nature and of such a strength as to make for a perception which results in the preference of the organism for a stimulus complex where the discontinuous members do in fact coalesce as opposed to a stimulus complex where the members are still, in some degree, discontinuous (p. 507).

The nature of these "forces," however, remains a mystery. It is again clear that this result cannot be explained simply in terms of summation of positive and negative cues. The fact that the originally positive stimulus complex is not preferred to a novel stimulus complex suggests that there is a cue analyzer, or coding system, which generates signals for solid lines which are of the same type, but are in some sense "perferred to" the signal for the originally positive shape. Perhaps the preference is based on greater redundancy in the signal for solid lines. Such an explanation has been given elsewhere (Dodwell, 1958b). Krechevsky's Group II did not show the same tendency to prefer solid lines to interrupted striations in test trials, but as he himself points out, their performance can readily be explained as a tendency to respond on the basis of brightness differences between stimuli (present both in training and test trials for Group II, but not for Group I).

#### STIMULUS CODING

These two examples suffice to emphasize the importance of developing

an adequate theory of stimulus analysis, or coding, before considering the role of S-R connections and response probabilities in discrimination learning. The arguments so far developed can be related to one of the sharper disputes in theoretical psychology, albeit a dispute which has not aroused much comment in the recent literature; namely the issue of continuity or discontinuity in learning. The protagonists of continuity have, on the whole, emerged from the dispute with more experimental support than their opponents (Ehrenfreud, 1948; Osgood, 1953; Spence, 1945), and it is certain that theoretical formulations in the continuity camp have been developed at a greater rate, and in more elaborate detail, as witness the rise of probabilistic theories of learning in the past few years. It has been argued here that these theories have been developed without regard for important considerations which, whilst not implying a theory of discontinuity, at least are close to the notions which led Lashley and his supporters to question the validity of theories of learning couched only in terms of incremental response tendencies. Thus, when Lashley developed his theory of selective attending, he was drawing attention to the fact that the way in which stimuli impinging on an organism's receptors affect that organism is an important consideration in developing theoretical models for learning. Lashley's key notion was that in some way the organism attends selectively to different aspects of the stimulus display; whilst this explains in general why some aspects of the display are more effective as discriminanda than others, it yields no specific prediction about which parts should be effective. There are really two issues here: (a) some stimuli appear to be more effective as discriminanda than others of the same

class (e.g., for the octopus and the rat straight lines oriented horizontally and vertically are more readily discriminated than straight lines in other orientations), and (b) some *classes* of stimuli are more readily responded to than others (e.g., in the rat brightness appears to be a more effective cue than shape). Coding systems (for shape, size, etc.) could explain facts of the first sort, and if specified in sufficient detail would yield predictions about the relative ease of different discriminations within a particular class of stimuli. If a series of coding systems were arranged in parallel, but in a hierarchy of control, this would account for b. If the hierarchy of control turned out to be flexible rather than fixed, "attention" might be identified with a selector for the coding system to control behavior at any given time. The problem of conditioning responses to whatever cues are attended to could safely be left to the "black box" of learning operations.

The precise form of the coding systems would depend on the facts to be explained (see below). It should be noticed that this position is not incompatible with the ideas of stochastic learning theorists, and indeed the two approaches should, perhaps, be thought of as complementary to each other. It has already been suggested that a stimulus coding system "hooked up" to a probability generating device is a possibility, and might indeed yield a very satisfactory model for discrimination learning. Including an appropriate system for stimulus coding would remove the ambiguities in the use of vague terms such as cue (and could account for known peculiarities of discrimination such as fixed preferences and certain transfer data), and a probability generating system would allow for changes in discrimination behavior as a result of reinforcement,

etc. Thus, the operations of "classifying" and "responding correctly" would be explicitly separated. Whereas "selective attending," or "coding," may influence the classification or recognition of patterns, it does not follow that this entails knowing how to respond to the classified pattern, and this may be learned in an incremental fashion. (In human learning, for instance of paired associates, this is obviously the case. The distinction between perceptual and response processes has been recognized in work on humans—Lawrence & Coles, 1954; Neisser, 1954).

If the writer's analysis is correct, then one could say that the different theoretical standpoints which led, among other things, to the continuity controversy are not mutually excluding, and that issues in discrimination learning raised by the parties to the dispute are not necessarily critical for those standpoints. Much of the evidence on either side is ambiguous, and open to more than one interpretation (Osgood, 1953). If coding of stimulus input should lead one to expect "discontinuous" discrimination behavior, it can just as well be argued that attaching responses to the coded input may only be learned incrementally.

#### SHAPE CODING SYSTEMS

Several proposals have been made in recent years for a coding or sorting system for shape in visually primitive organisms (Deutsch, 1955; Dodwell, 1957b; Sutherland, 1957). Deutsch devised a system which transforms spatial patterns of information into temporal patterns, so that each shape in general generates a distinctive temporal signal; such a system has the important property that recognition of spatial patterns, irrespective of their *location*, no longer presents such an intractable problem as in a system

which maintains spatial order. This problem has been one of the main stumbling blocks for neural theories about visual recognition (Dodwell, 1957a, 1958b). For instance, it is treated inadequately in gestalt-type theories (Koffka, 1935), and in Hebb's theory (Hebb, 1949). Pitts and McCulloch (1947) give a theoretically satisfactory account, but at the expense of great complication, which leads to criticism on anatomical grounds (Lashley, 1952). Whilst the coding theories are very much more restricted in scope than the theories just referred to, they have certain distinct advantages. The main advantages are that they yield clear predictions which can be tested, and they are compatible with what is known about the relevant neural systems. Two of them (the theories of Deutsch and Dodwell) also provide logically satisfactory bases for a theory of perceptual generalization. Both theories will account for recognition of shapes independently of size, location, or the destruction of large parts of the striate area (Deutsch's idea of transformation into temporal signals is also incorporated in the writer's theory) but differ sharply in other respects (Deutsch, 1958; Dodwell, 1958a). The writer's theory was designed to account, among other things, for: (a) effects of rotation in the frontal plane (Dodwell, 1957b, 1960; Fields, 1932; Sutherland, 1959b), (b) the fact that horizontal and vertical components of shapes are more easily discriminable than others (Dodwell, 1960, 1961; Sutherland, 1957), and (c) the fact that rats trained to jump to horizontal striations will subsequently transfer to other shapes with horizontal base lines (Dodwell, 1957b). It also accounts for the findings of Lashley and Krechevsky quoted above, in terms of mutual interference of two signals, and greater



redundancy of certain signals, respectively (Dodwell, 1958b).

Both these theories are oversimplifications, whatever their respective merits, since there are some facts about shape discrimination which are not explainable in either (Dodwell, 1960, 1961), and one must assume, of course, that other stimulus analyzers (e.g., for brightness) will be operative. But at least they represent an attempt to outline one possible way in which the theoretical treatment of the subject may be developed. Sutherland (1959a) has recently argued a case for such development, as opposed to very general theories of neural organization.

If the coding-type theories so far developed are not adequate to explain completely shape discrimination in relatively simple visual systems, such as those of the rat and the octopus, they will certainly be inadequate as a basis for the explanation of shape coding in humans. Nevertheless, there are indications that any theory about the neurological system underlying shape recognition in humans must account for orientational phenomena of a similar type to those found in more primitive visual systems. An attempt to describe and account for such facts in human vision in terms of a shape analyzer would lead too far afield at present, but it is hoped to tackle the problem in a subsequent paper.

#### CONCLUSION

The arguments presented above have been addressed to the problem of what "a stimulus" is, and to showing that failure to take account of the problem in stochastic theories of discrimination learning renders these theories implausible. They assume, implicitly, that the process of discrimination learning starts with an "unstructured" stimulus display, but experimental findings show that for discrimi-

nation learning in the adult organism this assumption is untenable. Evidence for the octopus and the rat mentioned earlier leads to the conclusion that—at least in shape discrimination—stimuli are classified in characteristic and often surprising ways. The findings are complex, and no satisfactory classification or coding system for shape (or any other property) has as yet been specified. It would seem desirable to identify what the system, or systems, are, before proceeding to the development of elaborate theories of discrimination learning. Experimental investigation of this topic has been started in recent years, the most extensive studies being those of Sutherland on the octopus (cf., for example, Sutherland, 1960). It would perhaps be premature to try to decide whether the results of such investigations will call for a major revision in the conceptual apparatus of stochastic learning theorists. The writer is inclined to think not. Insofar as the findings help to specify the events antecedent to the generation of response probabilities they should, however, lead to some revision of the details of stochastic models.

A frequent criticism of the stimulus coding theories (in private communication) has been that they take no account of perceptual learning. Conjunction with a stochastic model could perhaps answer this criticism. Whether such a happy marriage can be arranged is another matter, but certainly the possibility deserves consideration.

In conclusion, it may be pointed out that problems of classification are central to the progress of theoretical psychology. As Luce (1959) has said:

all of our choice theories . . . begin with the assumption that we have a mathematically well-defined set, the elements of which can be identified with the choice alternatives. How these sets come to be defined

for organisms, how they may or may not change with experience, how to detect such changes, etc., are questions that have received but little illumination so far. There are limited experimental results on these topics, but nothing like a coherent theory. Indeed, the whole problem still seems to be floundering at a conceptual level . . . (pp. 3-4). (Reprinted with permission from R. D. Luce, *Individual Choice Behavior*, 1959, John Wiley & Sons, Inc.)

It has been the aim of this paper to clarify some of the conceptual muddles, and to point out a possible line of advance for theories of discrimination learning.

#### SUMMARY

The problem of how a stimulus should be defined is raised, and the need for more adequate treatment of organisms' stimulus classification systems is pointed out. This is especially necessary in the field of shape discrimination. Neglect of the issues involved has led to ambiguity in modern learning theories; it is suggested that stimulus classification and changes in response probabilities are both important, and both should be dealt with in an adequate theory of discrimination learning.

Some empirical evidence is adduced to illustrate the importance of stimulus classification, or coding, and some attempts to devise shape coding systems are briefly discussed. The relevance of such systems to the problems of perceptual generalization and human perception is noted.

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## THE USE OF IONIZING RADIATION AS A MOTIVATING STIMULUS<sup>1</sup>

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In a series of experiments at this laboratory we have employed ionizing radiation as a motivating stimulus to elicit avoidance behavior in animals. The supposition underlying these studies was that exposure to ionizing radiation resulted in unpleasant sensations to which cues present during the exposure may be conditioned. Experiments based on this hypothesis have yielded consistent results at relatively low dose levels of radiation. As suggested by Arbit (1958a), the phenomenon may provide the psychologist with a new aversive stimulus having some unique properties; furthermore, it is of interest as a behavioral manifestation of biological responses to radiation exposure. The purpose of this paper is to review the published studies, provide new data, and discuss the situations in which conditioning can be obtained. Also, some speculations concerning the nature of the motivating stimulus are offered.

### CONDITIONED CONSUMMATORY BEHAVIOR

#### *Conditioned Avoidance of Food and Water*

The studies of conditioning at this laboratory received impetus from observations of food and water consump-

tion of rats during prolonged exposures to gamma rays from cobalt-60 (Garcia, Kimeldorf, Hunt, & Davies, 1956). The animals were exposed once per week to a dose of 75 r. delivered over an 8-hour period. Food and water consumption was depressed during the initial period of exposure and the degree of depression was greater with each succeeding exposure. Consumption between exposures was not reduced; indeed, the irradiated animals tended to compensate by drinking more water immediately following the exposure period. Hence this progressive decline did not appear to be an accumulative damage effect, rather it appeared likely to us that the animals were learning to avoid the consumption of food and water during the period of radiation exposure.

To test this hypothesis, animals which had previously experienced food and water consumption during a series of weekly radiation exposures were subjected to a sham irradiation test of food and water consumption (Garcia, Kimeldorf, & Hunt, 1956; Kimeldorf, Garcia, & Hunt, 1955). During this test the manipulative procedures were identical to an actual exposure except that a lead shield located near the source of radiation reduced the exposure dose virtually to zero. Once again the animals behaved as though irradiated, that is, they consumed less food and water than did previously sham irradiated controls in the same test situation. Additional observations revealed that if animals were deprived of food and water during radiation exposure they would consume normal

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amounts during a subsequent sham irradiation test. Hence it was apparent that the decrement in consumption was not attributable to radiation injury *per se* but was dependent upon the experience of ingestion coupled with radiation exposure.

#### Conditioned Aversion to Saccharin

The hypothesis that the temporal association of consumption and irradiation was a critical factor in the reduced consumption received confirmation in subsequent studies. Saccharin flavored water, in a concentration highly preferred to tap water by rats, was used as the conditioned stimulus in these experiments. The design of the first experiment (Garcia, Kimeldorf, & Koelling, 1955) was a six-fold classification of treatments in which groups of rats drank either saccharin flavored water or tap water in conjunction with either a sham exposure, a 30-r. or a 57-r. dose of gamma rays. Following a single treatment the six groups were tested for saccharin preference (saccharin fluid vs. tap water) in their home cages. The results of the initial postirradiation test are illustrated in Figure 1 and the extinction of the

effect is illustrated in Figure 2. The animals which drank saccharin flavored water during actual exposure exhibited a marked aversion for the fluid during the postirradiation tests, while the remaining groups continued to display a high preference for the saccharin solution. The effect was dose dependent and persisted for more than 4 weeks following the 6-hour radiation exposure.

The temporal relationships between the radiation exposure period and the saccharin drinking period were varied in order to determine how well this response to radiation fits the paradigm of conditioning (Garcia & Kimeldorf, 1957). Figure 3 shows the sequential design of the experiment and also illustrates the effect of altering the temporal relationships between saccharin presentation and radiation. The ratio of post- to preirradiation consumption of saccharin fluid was used to score the preference. Animals which drank saccharin during exposure (38-r. dose) exhibited the strongest aversion toward the solution in the postirradiation tests. Animals which drank saccharin prior to exposure (trace conditioning) also displayed an aversion. Conditioning did not occur in animals which drank immediately following the exposure. These results fit the paradigm of classical conditioning even though the presentation of the stimuli in these studies was measured in hours rather than in seconds.

That radiation induced conditioning may be due to a disturbing artifact coincident with radiation exposure has been eliminated by the technique utilized for sham irradiation of controls. During sham exposures the radiation was absorbed by lead shields or shutters near the source so that the control animals were fully exposed to all the stimuli from the machinery except radiation. The use of trace condition-

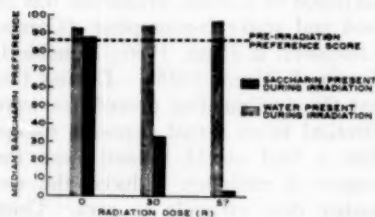


FIG. 1. A conditioned aversion to saccharin which resulted from drinking the solution during a 6-hour exposure to gamma rays. (The six groups of rats were tested for saccharin preference in their home cages 2 days after a single treatment under the conditions specified in the figure. The score is the amount of saccharin flavored fluid consumed in percentage of total fluid intake. Ten animals per group.)



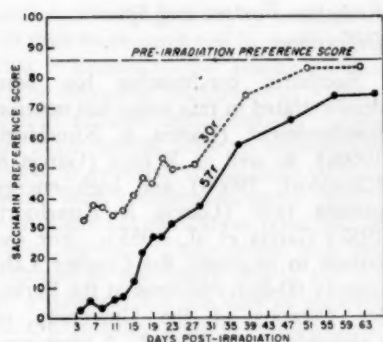


FIG. 2. The extinction of a conditioned aversion to saccharin which resulted from drinking the solution during a single exposure to ionizing radiation (Garcia, Kimeldorf, & Koelling, 1955). (The two groups of rats were tested in their home cages with saccharin solution and tap water continuously available.) (Reprinted with the permission of *Science*)

ing for the establishment of saccharin aversion similarly precludes the possibility that the aversion was a result of ionizing radiation having a direct action upon the test solution itself.

#### *Additional Data concerning the Response to Low Levels of Radiation*

In the previous experiments, avoidance conditioning of saccharin was shown by the measurement of saccharin preference in the home cage in the absence of other stimuli associated with radiation exposure. In a subsequent experiment, designed to provide a more sensitive test of conditioning, saccharin fluid consumption was measured during confinement to the radiation chambers as in the sham exposure test of food and water consumption (Kimeldorf et al., 1955). Under these circumstances wherein all stimuli conditioned to exposure would operate, it was possible to condition saccharin preference with very small radiation doses.

The apparatus, radiation source, and

animal stock have been described previously (Garcia & Kimeldorf, 1957). Three groups of 16 rats each were used. A supply of 0.1% saccharin solution was made available during each confinement to the radiation chamber. A preirradiation test of consumption was made during a 4-hour confinement to the radiation-exposure chamber. A single conditioning exposure was made 2 days later with gamma radiation from a cobalt-60 source. Group I (control) was pro-

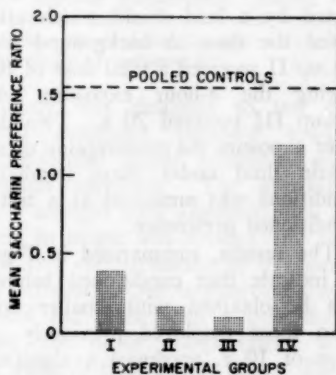
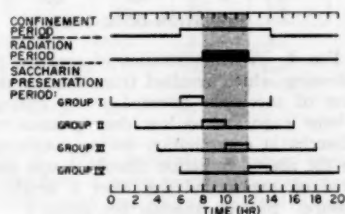


FIG. 3. Temporal relationships within the conditioning of a saccharin aversion through gamma radiation showing the experimental design and results (Garcia & Kimeldorf, 1957). (All animals were deprived of water for 6 hours prior to and again following the presentation of the saccharin solution. Saccharin preference test was made 3 days after the conditioning session. The score is the ratio of postirradiation to preirradiation consumption of saccharin flavored water. Ten animals per experimental group and 20 controls.)

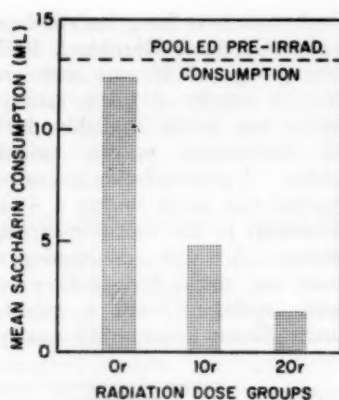


FIG. 4. A demonstration of aversion conditioning which resulted from the consumption of saccharin flavored water during a 4-hour exposure to low dose gamma rays. (Saccharin consumption during confinement to the plastic radiation chambers was measured on the second day after a single exposure. Sixteen animals per group.)

ected by a lead shield which attenuated the dose to background level. Group II received a total dose of 10 r. during the 4-hour exposure while Group III received 20 r. Two days after exposure the consumption of saccharin fluid under sham irradiation conditions was measured as a test of conditioned preference.

The results, summarized in Figure 4, indicate that conditioned behavior can be obtained with smaller doses than those employed previously. A dose of 10 r. produced a significant decrement in saccharin consumption ( $p < .01$ ) during the postirradiation test while 20 r. had a more pronounced effect.

A functional test which can reliably detect the effects of 10 r. delivered at the rate of 2.5 r. per hour is very sensitive. Under these conditions of exposure a dose greater than 1,000 r. would be required to produce any fatalities within a 30-day period.

### *Radiation Factors and Species Differences*

Saccharin conditioning has been demonstrated in rats using fast neutron bombardment (Garcia & Kimeldorf, 1960a), as well as X rays (Garcia & Kimeldorf, 1960b) and high energy gamma rays (Garcia & Kimeldorf, 1957; Garcia et al., 1955). For exposure to neutrons, the Crocker Laboratory 60-inch cyclotron at the Berkeley Campus of the University of California was employed. A trace conditioning method was used in which a 4-minute exposure to neutrons was preceded by a 20-minute presentation of saccharin fluid. A dose of 7.5 rad. produced in the exposed animals an 80% reduction in preference for the saccharin fluid as compared to the unconditioned irradiated controls. The 7.5-rad. dose is roughly 2% of a 30-day lethal dose for this type of radiation. In contrast to the previous studies which employed radiation exposures of several hours at low dose rates, the neutron exposure lasted only a few minutes. Thus conditioning can be obtained under a wide variety of ionizing radiations, dose rates, and exposure conditions.

Conditioned aversions have been established in cats and mice as well as rats following exposure to X rays (Kimeldorf, Garcia, & Rubadeau, 1960). The general method employed was to expose animals of each species while they were ingesting a distinctive fluid. For each of the three species the consumption of the test fluid was compared for sham irradiated and irradiated groups in a sham exposure test carried out after the last of three conditioning exposures (Figure 5). Cats were conditioned to chocolate flavored milk while rats and mice were conditioned to saccharin flavored water. The doses employed with mice

and cats were greater by a factor of 10 than those employed in conditioning rats. However, the results of this study cannot be interpreted as a measure of relative radiosensitivity among the three species since no attempt was made to establish comparative thresholds. Such comparisons would require equivalent discrimination tasks for all three species. Equivalence was not obtained in this study due to the use of a different CS for cats as opposed to the rodents, and, in addition, the species differed markedly in habituation to the experimental situation.

#### AVOIDANCE OF THE PLACE OF RADIATION EXPOSURE

##### *Spatial Avoidance Studies*

The radiation conditioned behavior in the studies described thus far has involved an inhibition of consumption. Another series of studies was designed to investigate avoidance responses in a situation which was independent of stimuli associated with consumption (Garcia, Kimeldorf, & Hunt, 1957).

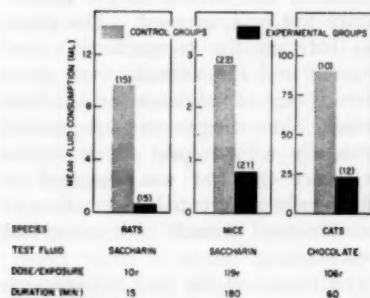


FIG. 5. Aversion conditioning of rats, mice, and cats as a result of drinking flavored fluids during three exposures to X rays. (Control groups drank the same fluids during sham irradiations. The bars depict the mean consumptions of the test fluids during a sham irradiation test on the third day following the last exposure. Group sizes are indicated by numbers in parentheses.)

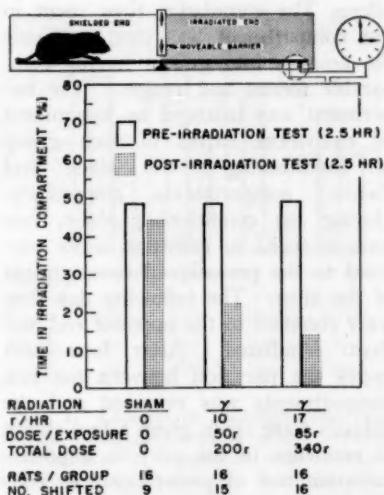


FIG. 6. Place avoidance behavior which resulted from a series of radiation exposures associated with one of two distinctive compartments. (The rats were sham irradiated in the opposite compartment. During the test periods the barrier between compartments was removed and the rats were given a free choice of residence.)

A study was designed to test for spatial avoidance behavior toward environmental stimuli associated with radiation exposure. The apparatus was a straight alley approximately 30 inches in length and divided into two distinctive compartments by a removable partition. One compartment was painted flat black with a grid floor, while the other compartment was flat white with a mesh floor. Each animal was always trained and tested in the same alley located in a standard position within the radiation exposure room to assure a multiple-stimulus learning situation. The cumulative residence time in the black compartment was recorded automatically (Figure 6).

A preirradiation test of residence preference was made which consisted of a free choice of residence in the

alley. The cumulative time spent in one compartment was used to divide the animals into groups having comparable means and ranges. The experiment was balanced by assignment of equivalent halves of each group for conditioning to the "black" and "white" compartments, respectively. During the conditioning phase, rats were exposed to radiation while confined to the preassigned compartment of the alley. The following day they were confined to the opposite end and sham irradiated. After four such cycles the partition between the two compartments was removed and the animals were again given a free choice of residence in the alley in a postirradiation test of preference. In contrast to sham irradiated controls the irradiated animals exhibited a decreased preference for the compartment in which they were exposed, demonstrating that radiation exposure can produce conditioned avoidance of a place (see Figure 6).

#### *Further Studies on Spatial Avoidance*

The original studies in spatial avoidance involved X and gamma radiation exposures of an hour or more. These studies have been extended at this laboratory in four experiments with very brief periods of exposure to X rays.

The animal stock, experimental procedure, and apparatus were similar to those used in the previous study (Garcia et al., 1957). Young male rats were habituated to the apparatus in two one-hour periods prior to experimentation. On the basis of a preirradiation preference test the animals for each experiment were equally divided into two groups of 16, having comparable medians and variabilities in residence preference. One group was irradiated in the black compartment and sham irradiated on the following day in the white compartment, while

the other group was treated conversely. The animals were subjected to a series of eight cycles of X ray exposure and sham exposure. The time between radiation exposures varied from 46 to 80 hours. All four experiments were conducted on identical schedules. For Experiments A and B the total dose per exposure was 25 r. (air dose) delivered at rates of 10 r/minute and 5 r/minute, respectively. The dose was reduced to one-half (12.5 r.) in Experiments C and D, while the rates were held constant. The radiation source produced X rays of 230 kvp with a half value layer of 2.7 mm. Cu.

During each conditioning trial, each animal was confined to the specific compartment for 10 minutes prior to the onset of the brief radiation exposure. Immediately after exposure the animal was removed from the compartment and returned to his home cage. On the next trial the animal was confined to the opposite compartment of the same alley for a similar period under sham irradiation conditions. The day after the final trial a postirradiation test similar to the pre-exposure test was imposed. The partition between the compartments was removed and the animals were given a free choice of residence for a 2-hour period. Since the groups were equated previously with respect to residential preference the test was designed to reflect only differential association of environmental stimuli with coincident irradiation.

The results of the four experiments are summarized in Figure 7. The differences within each experiment between the postirradiation test scores of animals irradiated in the white compartment and animals irradiated in the black compartment were tested with the nonparametric *U* test (Siegel, 1956). Eight conditioning exposures of 25 r. resulted in a significant effect ( $p <$

.001) whether delivered at 10 r/minute or at 5 r/minute. The use of 12.5-r. exposures produced a reliable effect ( $p < .01$ ) only when delivered at the higher dose rate of 10 r/minute. Analysis of change in residence preference scores from the pre-exposure test to the conditioning test yielded comparable probability values.

Despite marked changes in rate and dose, the present experiments confirm the original findings regarding spatial avoidance. The smallest radiation dose employed in the previous study of spatial avoidance was four exposures of 5 hours duration to gamma rays at the mean rate of approximately 0.17 r/minute with a resultant dose of 50 r. per exposure. Significant effects were obtained in the present study with eight X ray exposures of 1.25-minute duration at the mean rate of 10.0 r/minute. This dose was only 12.5 r. per exposure. As in the saccharin aversion studies, the rate and exposure durations apparently can be varied over a wide range without essential change in the phenomenon.

Positive results in spatial avoidance conditioning were also obtained by Arbit (1959b) who employed the basic technique (Garcia et al., 1957) to demonstrate that the radiation conditioning phenomenon was not due to noxious odors associated with radiation exposure.

In the first of a series of investigations, Rohles, Overall, and Brown (1959) failed to obtain spatial avoidance conditioning of rats with X rays under conditions intended to duplicate our original study (Garcia et al., 1957). In their study a generalization test of a single-stimulus discrimination, brightness contrast, was employed. Under the conditions used, a strong preference for the black compartment ( $> 80\%$ ) was observed prior to exposure. In contrast, the original study

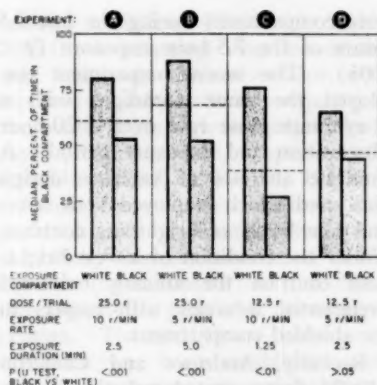


FIG. 7. Changes in residence preference for the black compartment which resulted from differential experience with radiation and sham irradiation in two distinctive compartments. (The groups were matched for residential preference for black before the series of exposures—dashed line. Conditioning series consisted of an alternating sequence totaling eight exposures and eight sham exposures. Sixteen animals per group in each experiment.)

(Garcia et al., 1957) utilized a multiple-stimulus discrimination (flooring, albedo, and environmental cues), each animal was conditioned and tested in the same apparatus under low illumination, and no marked preference for the black compartment occurred in the preirradiation test ( $< 70\%$ ). It is probable that the resultant discrepancy between these two studies stems from these differences.

In two experiments Overall, Brown, and Logie (1959) and Overall, Logie, and Brown (1959) employed an instrumental escape paradigm with rats in which the animals had free access to either the white or black compartments of a shuttle box with the white end shielded from the X ray source. In the first experiment the groups exposed to either 1 or 2 r/minute showed a trend in residence preference away from the black radiation expo-

sure compartment during the last 2.5 hours of the 7.5-hour exposure ( $p < .005$ ). The second experiment employed the same paradigm with a 1 r/minute dose rate over a 20-hour observation and exposure period. A multiple analysis of variance design was used which employed both sexes and five levels of brightness contrast. Under the condition of lowest brightness contrast the animals exhibited preferential behavior with respect to the shielded compartment.

Recently, Andrews and Cameron (1960) demonstrated radiation avoidance behavior in the mouse. They found that, in the absence of differential cues, mice will spend less time in that portion of an alley which is exposed to radiation than in the shielded end.

The relative ease of demonstrating the conditioning phenomenon may depend greatly upon methodological features of the conditioning situation. The escape paradigm employed by

Overall and his co-workers and by Andrews and Cameron reflects choice behavior in a free-trial situation in which latency and duration of stimulation cannot be controlled readily and which tacitly assumes termination of radiation induced motivation as the animal passes from the exposed to the shielded compartment. Though procedurally more complex, the avoidance conditioning paradigm employed by Garcia, Kimeldorf, and Hunt (1957) and by Arbit (1959a, 1959b) involves forced-trial conditioning which allows direct control of stimulation and avoids precise assumptions about the duration of the motivation state induced by radiation.

#### OBSERVATIONS CONCERNING THE UCS

Several attempts have been made to define experimentally the physiological mechanism underlying the phenomenon using conditioned aversion towards saccharin as an end point.

A trace conditioning technique has been employed and the relative radiosensitivities of various anatomical regions for the induction of conditioned behavior were explored with a collimated X ray beam  $\frac{3}{4}$  inch in diameter (Garcia & Kimeldorf, 1960b). Four X ray doses were used in conjunction with each of four sites of exposure which resulted in a 16-group design. These groups were compared with non-irradiated and with total-body exposed groups in the conditioning test of saccharin consumption (Figure 8). A dose of 54 r. elicited a decrement in postirradiation saccharin preference when the beam was directed at the abdomen. The decrement was not as pronounced as that observed following whole body exposure with the same dose. The 54-r. dose had no appreciable effect when the beam was directed towards the head, thorax, or

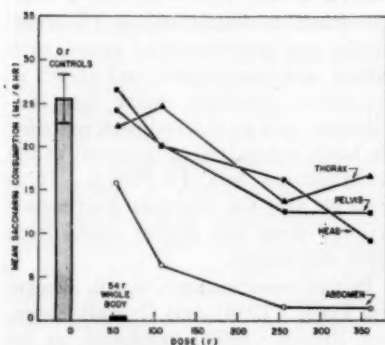


FIG. 8. Aversion conditioning as a result of localized X irradiation associated with saccharin flavored water (Garcia & Kimeldorf, 1960b). (The consumption of the test fluid was measured in the home cage on the second day following exposure. Standard error of the mean is indicated for the control group. Each point represents the mean for 14 rats.) (Reprinted with the permission of *Radiation Research*, Academic Press, Inc.)



pelvis. However, reliable depressions in saccharin consumption were produced when the dose to these regions was raised to 252 r. Therefore, while the abdomen proved to be the most sensitive area, conditioning can be instigated by irradiation of other regions with higher doses.

Radiation conditioning has a marked resistance to interference by surgery or drugs. Neither adrenalectomy nor hypophysectomy prevented the conditioned aversion towards saccharin (Garcia & Kimeldorf, 1960b). Arbit (1959a) found that autonomic blocking agents (tetraethylammonium and hexamethonium) did not interfere with radiation conditioned spatial avoidance in contrast to their apparently detrimental effects on learning to escape from electric shock (Arbit, 1958b).

Contrary to popular belief the direct perception of radiation under special conditions has been established in several diverse species. Lipetz (1955) reviews early X ray studies which demonstrated that positively phototropic insects and crustaceans moved towards an ionizing radiation source except when blinded. Under conditions of dark adaptation X ray doses of less than 1 r. will produce a phosphene which in human subjects has been described as a greenish luminous glow in the visual field (Godfrey, Shenk, & Silcox, 1945; Newell & Borley, 1941). Dawson and Smith (1959) found that doses of 1-25 r. would produce marked visual sensitization as measured by threshold shifts of the single visual receptor in the lateral eye of *Limulus*. References to sensations other than retinal can also be found. The recent work of Otto Hug (1958) suggests that ionizing radiation may be an effective stimulus for chemoreceptors in invertebrates at extremely low doses. Water fleas have been observed to respond promptly to

ionizing radiations (Baylor & Smith, 1958). A smarting sensation of the skin and a discriminative odor were reported by subjects briefly exposed to the scatter from high intensity cathode rays (Robbins, Aub, Cope, Cogan, Langohr, Cloud, & Merrill, 1946).

While these reports indicate that radiation may be directly perceived it is unlikely that the motivation in the conditioning studies is due to a direct effect upon the peripheral sensory receptors. The conditioned aversion to saccharin has been established with radiation exposure in ophthalmectomized rats (Garcia & Kimeldorf, 1958). In addition, most of the saccharin experiments previously described were conducted under conditions of illumination not conducive to the phosphene effect. Thus the retina can be eliminated as an essential site of action. Injury to gustatory function cannot account for the saccharin aversion since animals exposed to radiation without contiguous presentation of saccharin fluid demonstrated a normal postirradiation preference for the solution. As previously described conditioning has been achieved with radiation exposure restricted to small areas of the body (Garcia & Kimeldorf, 1960b). Since the abdomen was found to be a differentially sensitive area of the body it does not appear that direct action of radiation upon skin receptors is the basis for conditioning. Similarly, direct effects upon receptors located in the head cannot be the cause since conditioning was established in animals with the head shielded from exposure.

When rats undergo a series of exposures as in a spatial avoidance problem, there is no evidence of excessive urination, defecation, or squealing such as usually occurs when painful stimuli are applied. During the postirradiation tests in the spatial avoidance studies most animals made a number of ex-

ploratory entries into the radiation compartment, suggesting perhaps that occupancy of the compartment is only mildly noxious, or that the animal is unsure as to the source of his discomfort. Thus, the rat's behavior differs markedly from that which is observed when an obtrusive stimulus (i.e., electric shock) is used to elicit an avoidance response.

#### SPECULATIONS REGARDING THE UCS

When an animal is placed in a field of ionizing radiation a sequence of events occurs which is initiated with the absorption of energy by the tissues and subsequently leads to gross adaptive reactions by the organism to the radiation insult. When radiation energy is transferred to atoms the position and relationship among the planetary electrons is disturbed. Excitation occurs if electrons are displaced from stable orbits to higher energy orbits while ionization results from the removal of an external orbital electron. Damage can be caused directly by the action of radiation upon organic molecules, however in biologic systems the indirect effect due to ionization of water is considered to be of greater importance. Ionization of water molecules produces highly reactive substances, mainly oxidants, which interfere with cellular metabolism and growth. The induced biochemical lesion represents a challenge to the homeostatic stability of the organism. The subsequent adjustments are reflected in the function and behavior of the exposed individual. Aversive behavior, conditioned by radiation exposure, is a manifestation of these events.

In this particular adjustment it is not known whether the nervous system is affected directly by radiation or is responding to radiation induced damage in other physiological systems by

means of a humoral mechanism. A prompt effect of radiation upon gastric retention (Jones & Kimeldorf, 1959) and a subsequent anorexia (Smith & Tyree, 1954) have been observed with relatively small doses of radiation. These symptoms can be evoked by local irradiation of hind limbs, chest, or head (Smith & Tyree, 1954; Swift, Taketa, & Bond, 1955). Since conditioning has been achieved by local exposure (Garcia & Kimeldorf, 1960b) it is possible, as in the case of gastric dysfunction, to postulate a humoral mechanism. However, a direct neural mechanism cannot be eliminated. Conard (1956) and others (Swann, 1924; Toyama, 1933b) employed surgical and pharmacological techniques to indicate that radiation can act directly upon central and/or peripheral autonomic centers to produce gastrointestinal disturbances. Radiation exposure also has been found to alter blood pressure, heart rate, and respiration rate of rabbits (Brooks, Gerstner, & Smith, 1956; Toyama, 1933a) during exposure and allows the inference of direct action upon neurological control. It is difficult to explain conditioning on the basis of direct radiation action on neural tissue since the dose levels employed for studies indicating direct neurological responses exceed by several magnitudes the dose necessary for conditioning behavior. Also localized irradiation of the head was no more effective than irradiation of the thigh in establishing a conditioned aversion toward saccharin (Garcia & Kimeldorf, 1960b). The results of the conditioning studies are more compatible with a hypothesis of humoral mediation, however the issue is not yet resolved.

The conditioned avoidance of food, water, or saccharin should not be confused with the postirradiation anorexia

observed in rats following relatively heavy exposure doses as in the interpretation by White and Brown (1958). In their study animals which received a dose in the lethal range (1,200 r.) avoided food and the investigators interpreted these results as evidence that avoidance behavior is not dependent upon conditioning. Doses of this magnitude result in progressive distention of the stomach with fluid, essential cessation of propulsive motility in the gastrointestinal tract, and cause extensive lesions of the mouth and gastrointestinal tract (Conard, 1956; Quastler, 1956; Smith, Ackermann, & Smith, 1952). Hence it is not surprising that the animals avoided food without previous conditioning. Studies which employ near-lethal amounts of radiation to study conditioned consummatory behavior would be complicated by direct radiation interference with gastric function. The saccharin conditioning studies involved low doses and contained the critical control group which was irradiated but not conditioned, i.e., did not drink saccharin during exposure. In contrast to the conditioned animals, these irradiated animals drank copious quantities of saccharin flavored water in the postirradiation tests.

Similarly the conditioned avoidance behavior is not primarily dependent upon the postirradiation malaise which is usually manifested hours after exposure and may persist for days (Castanera, Jones, & Kimeldorf, 1959; Jones, Kimeldorf, Kubadeau, Osborn, & Castanera, 1954). Furchtgott's (1956) plausible suggestion, based on the earlier studies which involved prolonged exposures, was that this phase of radiation sickness may have produced the saccharin aversion. However, in view of the evidence in the more recent studies concerning the

temporal relations within the conditioning sequence and the effects of brief exposures it is apparent that the motivating effect operates so promptly and at such low doses that it cannot be attributed to the general postirradiation malaise.

Although studies of responses conditioned with ionizing radiation have been limited to overt behavior (i.e., drinking, spatial avoidance) it should also be possible to condition responses involved with internal regulation. In a recent review Livshits (1956) cites some studies which have led Russian investigators to believe that the X radiation situation can operate as a conditioned stimulus to evoke changes in hematological indices of animals which had been previously exposed. During a postirradiation period when hematopoietic functions had returned to normal, sham exposure produced changes in several indices among which lymphopenia and leukopenia were observed. The effect was interpreted as the reproduction of radiation effects by the conditioned reflex route. Unfortunately, the complete experimental conditions and data are not included in the review. The findings cited by Livshits are congruent with the interpretation that internal responses can be conditioned by radiation exposure.

While the motivating stimulus is obscure and how it operates is still debatable, there can be no question that relatively small doses of ionizing radiation can serve to establish conditioned behavior. Physiological reactions to radiation are diverse and probably involve both humoral and neural mediation. The effects are reflected promptly in cardiovascular, respiratory, and gastrointestinal functions. It should not be surprising that behavior is a highly sensitive response to radiation exposure since slight

changes in several functions may act synergistically to provide the motivational qualities of the stimulus.

A review of the prevailing evidence suggests that the unconditioned stimulus may be closely related to the initiation of gastrointestinal dysfunction. Gastric dysfunction is discernable during the radiation exposure period (Jones & Kimeldorf, 1959). A decrement in gastric transit appears to coincide with the onset of radiation while a depression in gastric motility occurs with a latency of approximately 4 minutes. Unpleasant sensations associated with gastric disturbances which have latencies of this order could serve as the motivating stimulus. Results of avoidance conditioning studies and those of gastric dysfunction are parallel in other respects as well. Both exhibit a high degree of radiosensitivity. With exposures of limited regions of the body, both show the same pattern of differential sensitivity. Although the abdominal region is most sensitive, the responses may be produced with larger doses by exposure of any limited region (Garcia & Kimeldorf, 1960b).

The extreme sensitivity of the saccharin aversion method for the demonstration of radiation conditioning is consistent with the hypothesis that the motivating stimulus is an unpleasant feeling state associated with the initiation of gastric dysfunction. It is reasonable to assume that this feeling state would be more readily associated with taste sensations than with distal environmental cues since the radiation stimulus does not appear to involve receptors that normally give the animal information with regard to spatial orientation. Presumably the animal is required to utilize sensations associated with gastric disturbances in the solution of the spatial avoidance problem.

This suggests that ionizing radiation may provide a readily controlled tool for the study of perception and motivation in which the stimulus bypasses the usual sensory channels.

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## ANXIETY, PAIN, AND THE INHIBITION OF DISTRESS<sup>1</sup>

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Theories of anxiety have been developed from evidence as diverse as the avoidance behavior of animals and the symptomatic behavior of human neurotics; the language of these theories ranges from existentialism to learning theory. For all the differences in detail, however, there is remarkable similarity in the approach of different theorists to the problem of anxiety.<sup>2</sup>

We will, in the present paper, examine the proposition that these theoretical communalities do not fully encompass available data about human and animal distress and then go on to present several theoretical propositions supplementary to current theories of anxiety.

Briefly and without extenuation, the following shared characteristics of contemporary theories of anxiety can be noted. First, there exists an archetypical event or class of events which

evokes anxiety primitively or innately or congenitally. For Freud (1936), this original inciter was overstimulation; for Mowrer (1939), it is pain; for Miller (1951), the "innate fear reaction"; for Rank (1929), birth trauma; for Selye (1956), stress; for the existentialists, it is the very fact of being human and alive. The second communality in theories about anxiety is the postulation that, somehow, the response to the archetypical event is transferred to previously innocuous events, events either in the external environment or in the action of the organism. The typical assumption has been that this association takes place with contiguous occurrence of trauma and neutral event, although the students of human learning have been more detailed than this in discussing the conditioning of fear (see, for example, Dollard & Miller, 1950). Finally, it is assumed that the events terminating or reducing anxiety are closely related to the events which evoke it. Thus, the primitive danger of overstimulation is controlled by a reduction in level of stimulation; similarly, the "fear" of electric shock is reduced by moving away from events associated with shock, presumably in inverse analog to the model of hunger and thirst, where a deficit of some substance (deprivation) is repaired by its replacement (eating or drinking).

These common elements of present day conceptions of anxiety—the archetypical evoker, the mechanism for association to previously neutral events, and the parallelarity of the elicitation and the reduction of anxiety—have produced discernable biases in contem-

<sup>1</sup> Parts of this paper were read at the 1960 meetings of the Western Psychological Association at San Jose, California; it was written during the authors' stay at the Center for Advanced Study in the Behavioral Sciences, Stanford, California, and was prepared in relation to USPHS Research Grants M-1787 and M-2442.

<sup>2</sup> "Anxiety" has come to be one of psychology's umbrella constructs; it covers so wide an area of research and speculation that no precise specification of its usages is possible. Nonetheless, as in the case of "learning" or "perception," "anxiety" is a useful summary expression for a set of different but related observations (see Mandler & Kessen, 1959, for a discussion of problems in the definition of theoretical terms). The theoretical differentiations which are proposed here are held to be relevant to the study of avoidance behavior, of physiological indicators of visceral disturbance, and of reported phenomenal distress.



porary psychology. In theory, in research, and apparently in therapy, the problem of anxiety has come to be, on one hand, largely a problem of trauma—that is, what events set off the anxiety—and on the other hand, largely a problem of flight—that is, what responses will lead away from the inciting event. In what follows, we will examine the place of the “trauma” or “archetype” notion by examining in detail the best candidate for primary primitive evoker of anxiety—pain—and then we will go on to a consideration of a position that is alternative to, but not necessarily incompatible with, the common elements of anxiety theory sketched out here.<sup>3</sup>

#### DEATH OF PAIN

We will defend the position—coming to be widely held in American psychology—that a theory of anxiety based solely on pain as an archetypal precondition is untenable. The evidence at hand suggests two conclusions: first, that pain is not a necessary condition for the development of anxiety and avoidance behavior; and second, that when pain is apparently a sufficient condition for the development of anxiety, there is at work a variety of factors rather than a single innate link.

There are three areas of evidence that support the conclusion that anxiety can occur even when pain does not occur. First, there are external events other than pain which arouse, without prior experience of association with pain, behavior which bears the marks of distress or anxiety. Of particular interest to our argument in the next

section are the startle and distress responses of the newborn human infant to loud noise or to loss of support (Peiper, 1956; Watson, 1919). Among animals, escape, avoidance, and species-appropriate signs of distress to nonpainful events have been reported in abundance by ethologists; the mobbing of chaffinches at the appearance of an owl reported by Hinde (1954) is an example in point. Unless a severe twist is given to the behavioral interpretation of “anxiety,” these cases, among others, stand against the Original Pain principle.

More striking as a demonstration of the separability of pain and anxiety is the behavior of human beings afflicted with congenital analgesia. This apparently inherited syndrome consists typically of a complete absence of pain sensitivity despite otherwise normal registration of the environment. A review of 30-odd cases reported in the literature (Fanconi & Ferrazzini, 1957) shows the severely debilitating effects accompanying the absence of pain mechanisms. The patients are usually discovered to be mutilated during childhood; undiscovered fractures, scarred tongues and limbs, are among the injuries found. Despite the fact that these patients fail to develop specific adaptive avoidance behavior in the face of many injurious and noxious situations, anxiety toward other—non-painful—events always seems to develop normally. The conclusion applied to one such case by West and Farber (1960) can be generalized to all observed cases of congenital analgesia: “anxiety plays a motivating role in determining certain aspects of the patient’s behavior.” In brief, the development of anxiety and avoidance behavior is not halted by the absence of pain sensitivity, even though the avoidance of normally painful events is absent.

<sup>3</sup> No attention will be given to the problem of individual differences in anxiety (Mandler & Sarason, 1952; Taylor, 1953); presumably they can be represented, at least theoretically, as parametric variations of the general theoretical formulations which we are considering in this paper.

The foregoing two points have shown that distress will develop in the absence of pain. A third collection of evidence supports the assertion of the disjunction without conclusively demonstrating the absence of an association with pain, but the data, when seen all in a row, strongly indict an exclusive commitment to a pain-traumatic theory of anxiety. We refer here to the occurrence of anxiety or discomfort when highly practiced and well organized responses are interrupted. The early research of Lewin and his students and that of more recent workers (for example, Lewin, 1935; Marquis, 1943) suggest that the interruption of highly motivated, well-integrated behavior arouses emotional responses much like anxiety. To these data can be added the research on emotional responses of animals to frustration (for example, Marx, 1956). Similar and perhaps more revealing phenomena can be observed in young infants where, usually after and rarely before the sixth month of life, both the appearance of a stranger and the disappearance of the mother can give rise to signs of extreme distress. The fear shown by chimpanzees when confronted with the severed heads of other chimpanzees (Hebb, 1946) is another case which falls into this category of distress consequent on extreme perceptual discrepancy. It is at least difficult to fit these cases to a theory of anxiety which depends primitively on pain or any other archetypical trauma.

If it can be agreed that pain is not a necessary condition for the development of anxiety, another question comes to the fore. To what degree or in what fashion is pain a sufficient antecedent condition for the development of anxiety? The skeptical answer that appears to be warranted by the evidence is that the relation between pain and anxiety is rarely simple

or obvious, and further that attention to the distinction between pain as a sensory event and the distress reaction which usually but does not always accompany pain may clarify the complexity somewhat. The presentation of this line of discourse is made easier by the recent appearance of a stimulating review by Barber (1959) of problems associated with pain. We will, therefore, only summarize what seem to be legitimate supports for the two-or-more-factor theory of pain and then move on to a more extended treatment of the nature of distress.

There is some, though admittedly very little, evidence that the appearance of discomfort with painful stimulation requires early experience of as-yet-unknown character. Puppies raised by Melzack and Scott (1957) in a restricted environment showed indifference to stimulation painful to normal dogs and great difficulty in learning to avoid objects associated with pain. These observations are of crucial importance to speculations about anxiety and warrant replication and extension. In human infants, there is a striking temporal difference between the first "defensive" response to painful stimulation (withdrawal or startle) and the second "distressful" response (crying, increased motility, and so on). Peiper (1956) reports that the first response has a latency of 0.2 second while the second response has a latency as high as 5-7 seconds.

A similar separability of what might be called cognitive pain and distress occurs in some cases of prefrontal surgical interference to deal with intractable pain. Barber (1959), in reviewing the evidence, concludes:

When prefrontal leucotomy alleviates intractable pain it does not necessarily elevate the pain threshold or alter "the sensation of pain." . . . [Further,] with few, if any, exceptions, investigators report that the "sen-

sation" or "perception" of pain is practically unaltered by any of these procedures (p. 438).

Finally, Barber suggests that noxious painful stimulation has wide cortical effects and argues against a neurology of pain based exclusively on specific pain pathways or pain areas. The discomfort-pain association seems to depend on extensive cortical organization—in the words of the present argument, on experience of pain and discomfort.

The death of pain as original in all anxiety does not rule out alternative formulations of the traumatic or archetypal variety. Solomon and Brush (1956), for example, have taken students of aversive behavior to task for neglecting the investigation of noxious stimuli other than electric shock. When they ask, "Are all aversive anticipatory states alike?" they point to one alternative suggested by the elimination of pain as the sole antecedent of anxiety. Another alternative, which will be explored here, is to examine a postulation of anxiety which is independent not only of pain, but of any archetypal traumatic event.

#### NATURE OF FUNDAMENTAL DISTRESS

It is our contention that a nontraumatic theory of the sources of anxiety can be defended and, further, that anxiety may be reduced or terminated by devices other than escape from and avoidance of threat. These alternative formulations are proposed as supplements to, rather than as substitutes for, the archetypal theories of anxiety.

The schematic model suggested here for the occurrence of anxiety—in distinction from the classical model of the organism fleeing the associations of pain—is the cyclical distress of the human newborn. There may be antecedent events which could account for

the crying and increased activity we recognize as distressful in the young infant—for example, food privation, shifts in temperature, and so on—but *it is not necessary to specify or even to assume such a specific antecedent event.* It is a defensible proposition that the strong bent of the archetypal formulations to study those conditions of distress for which a specific evoker could be discerned seriously limits the range of proper investigation. The distress of the human newborn, as obviously "anxious" as a rat in a shuttle box, can be taken as an example of human anxiety and as a starting point for changes in speculations about human emotion, regardless of the absence of known or well-guessed "unconditioned" archetypal evokers. More than that, this modification suggests that there are cases in which the old and respected saw about anxiety as ~~the~~ conditioned form of the stimulus-specific fear reaction may be misleading; that is, there may be interesting cases in which a stimulus-specific fear (as indicated by flight or avoidance) may be better understood as a conditioned form of primitive anxiety or *fundamental distress*.<sup>4</sup>

To see anxiety as fundamental distress raises the ghosts of an old dispute in psychology—that between James and Cannon on the nature of emotion. Let us take a further theoretical step and suggest that the crucial event in fundamental distress is the perception or afferent effect of variable and intense autonomic, visceral activity. This is a rough restatement of James' position that emotions are the result of the perception of visceral events or are those perceptions themselves (James, 1890). Most of Cannon's counter-

<sup>4</sup> Auersperg (1958) has presented a treatment of "fundamental anxiety" (*Fundamentalangst*) which bears on the present discussion.

arguments to such a position are not relevant to the postulation of such an effect during early infancy, since his position depends to a large extent on the identification of external threatening stimuli—a feat beyond the powers of the newborn (Cannon, 1927). But Cannon's major argument that emotional reactions take place with a latency far shorter than the latency of autonomic reactions deserves particular attention here. The delayed emotional response of the infant cited earlier, as well as the variable, badly organized reactions of infants, suggests just such a delayed emotional mechanism as Cannon ascribes to James. If we assume further (cf. Mandler & Kremen, 1958) that these visceral reactions are eventually represented centrally (in other words, that "central" anxiety short-circuits visceral events), then ascription of a developmental shift from a Jamesian to a Cannonian mechanism becomes plausible.<sup>8</sup> A closely related point was recently made by Schneirla (1959):

although the James-Lange type of theory provides a useful basis for studying the early ontogeny of mammals, . . . a Cannon-type theory of higher-center control is *indispensable* for later stages of perceptual and motivational development. If ontogeny progresses well, specialized patterns of [approach] and [withdrawal] . . . , or their combinations, perceptually controlled, often short-circuit or modify the early viscerally dominated versions (p. 26).

One final comment on the nature of distress is warranted. It is not as-

<sup>8</sup> The argument that visceral discomfort may become centrally represented does not necessarily imply that the visceral response will not thereafter occur; the postulation of central representation is required to explain the quick and efficient reaction of the adult to threatening events. However, given the possibility of rapid removal from the situation of threat, the "postthreat" visceral response may in fact not occur. Recent research by Solomon and Wynne (1954) supports a similar interpretation.

sumed that the distress reaction is usually terminated suddenly by the occurrence of an escape or of an avoidance response. Rather, we assume that, except for a few laboratory situations, the distress reaction is reverberatory in character. Particular events or responses do not terminate the anxiety immediately; moreover, the distress reaction will serve as a signal for further distress. Depending on partially understood environmental and organismic conditions, these reverberations will augment the initial anxiety (see Mednick, 1958) or gradually damp out and disappear.

In short, fundamental distress is held to be a state of discomfort, unease, or anxiety which bears no clear or necessary relation to a specific antecedent event (archetypical evoker). The model or "ideal case" of fundamental distress is held to be the recurrent distress of the human newborn. Examination of the notion of anxiety in the light of these propositions is compatible with a resolution of the conflict between James' and Cannon's views on the nature of emotion. What remains for consideration is an examination of the occasions of reduction or termination of anxiety and the relation of such occasions to fundamental distress.

#### INHIBITION OF ANXIETY

The second departure from conventional views of anxiety has to do with techniques for the reduction or termination of anxiety. It is proposed that, in addition to the classical mechanisms of escape and avoidance of danger, anxiety is brought under control (that is, diminished or removed) by the operation of *specific inhibitors*. Before moving on to a discussion of the inhibitory mechanism, however, we must emphasize a point that is implicit in the foregoing treatment. The undifferentiated discomfort of the infant

which we have taken as an example of fundamental distress may accompany particular conditions of need or drive; that is, the newborn may be hungry *and* distressed, thirsty *and* distressed, cold *and* distressed, and so on. With the removal of the privation or drive, the distress may disappear, but this reduction by the repair of a deficit—which is formally equivalent with escape from danger—is not of primary interest in the present discussion. Rather, our concern is with those responses of the organism and events in the environment which inhibit distress, *regardless of their relation to a specifiable need, drive, or privation.*

Anecdotal evidence of the operation of congenital inhibitors of anxiety in infants abounds, but there has been relatively little systematic exploration of these inhibitors in the newly born, human or animal. However, two recent empirical studies will serve to illustrate the character of the inhibitory mechanism; one of them is based on a response of the infant, the other on a particular pattern in the environment. Research by Kessen and his associates has shown that infant distress, as indicated by crying and hyperactivity, is dramatically reduced by the occurrence of empty—that is, nonnutritive—sucking as early as the fourth day of life. The performance of the congenital sucking response on a rubber nipple stuffed with cloth brings the newborn to a condition of motor and vocal quiescence. Thus, sucking appears to fit the pattern of the congenital inhibitor of distress, or, more broadly, of anxiety. Systematic observation of the effects of sucking on motility in the period immediately after birth will be necessary to demonstrate that the inhibition is not “secondary” to the experience of food. There can be cited the incidental observation that the hungry infant during the first days of

life, with little or no experience of feeding, will quiet when given breast or bottle, even though it is unlikely that his hunger has been reduced during the first several sucking responses.

The second instance of distress-inhibition derives from Harlow's (1958) research with infant monkeys. These animals when distressed, whether by a frightful artificial Monster Rhesus or in the routine cyclicity of discomfort, seek out a situation—the experimental “mother”—which inhibits the distress. Harlow has made some provocative assumptions about the characteristics of the model which serve to reduce the infant monkey's distress and he has established an empirical procedure for testing them. What seems beyond doubt are the facts that a complex environmental event serves to terminate a condition of the animal that meets our usual criteria for the presence of anxiety and that this event bears no obvious relation to physiological privation or deficit.

There are undoubtedly several congenital or early developed inhibitors of distress which have not received adequate empirical examination; the quieting effects of rocking and the response of the 2-month-old infant to the adult face come to mind. A strong presumptive case can be made for the operation of a class of such distress terminators which do not depend for their effects on escape from or avoidance of an archetypical or traumatic evoker of distress.\*

\* One group of inhibitors of distress appears to be characterized by rhythmic periodicity: regular sounds, rocking, the nodding head of the adult, and so on. Investigation of the relation of this class of events to visceral rhythms would lead to increased precision in speculations about fundamental distress. It is interesting to speculate in this connection about the relation between distress-inhibition on one side and sympathetic-parasympathetic incompatibility on the other.



There is a further aspect of the problem of distress-inhibition which will illustrate the relation of fundamental distress and its inhibitors to anxiety of the archetypal variety. If distress is under control by the operation of an inhibitor, what is the effect of withdrawing the inhibitor? What, in other words, are the consequences of disinhibition of distress? For some occurrences of some inhibitors—for example, rocking the hungry and distressful infant—it seems that disinhibition “releases” or “reinstates” the distress. For others—for example, sucking on the hands until asleep—the withdrawal of the inhibitor does not result in the recurrence of distress.

The following proposals can be made to deal with this kind of disjunction. Archetypal evokers (for example, pain, hunger) are accompanied by or lead to distress. This distress can usually be reduced in two quite distinct ways: by action of a specific inhibitor which reduces distress but does not necessarily affect the primitive evoker; or by changes acting directly on the level of the primitive evokers. The best example of how these mechanisms work together in nonlaboratory settings is nutritive sucking. The infant's *sucking* inhibits the fundamental distress accompanying hunger; at a slower rate, the *ingestion of food* “shuts off” the source of distress. It is maintained here that these two mechanisms for the reduction of distress or anxiety are profitably kept separate in psychological theory.<sup>7</sup>

The separation of distress reduction by specific inhibition and distress reduction by changes in archetypal evokers can be defended on other grounds as well. As noted earlier,

<sup>7</sup> These postulations are formally equivalent to the theory Deutsch (1960) has proposed to account for behavioral phenomena associated with hunger and thirst.

much infantile (and later) distress is of a periodic variety without obvious relation to specific environmental evokers. Specific inhibitors may serve to tide the organism over the peaks of these distress cycles, whatever their source, until some other occurrence (for example, the onset of sleep) results in a more stable reduction of the level of organismic disturbance.

It is reasonable to assume that the inhibitory mechanism under discussion is not limited to the operation of primitive inhibitors early in development. Rather, events associated with inhibitors may, under appropriate circumstances, acquire learned or secondary inhibitory properties. Under this proposal, it can be maintained that the immediate “satisfying” effects of food may be ascribable to its association with the inhibition of distress by eating, rather than the other way round. F. D. Sheffield (unpublished) has proposed a mechanism for reinforcement which is closely akin to this argument.

With the foregoing reservations in view, we would argue finally that among the earliest differentiations the child makes are those that have to do with the handling of distress. Whether in regard to what we have called fundamental distress or in regard to distress set off by specific environmental events, much of early infant behavior can be related to the management of discomfort or unease. Furthermore, it is probably in these connections that the infant first learns about the consequences of interruption of organized response sequences or expectations. Just as it has been assumed that secondary inhibitors of distress can be developed, so it is assumed that learned signals of disinhibition—that is, the reinstatement of distress at the withdrawal of an inhibitor—can be developed over the course of in-



fancy. Thus, the phenomenon of separation anxiety seen in the young child can be understood as the interruption of well-established inhibitory sequences. The failure of the mother to appear, that is, the omission of an important inhibitor, leads to the rearousal of distress.\*

In short, anxiety is not only the trace of a trauma which must be fled, but is as well a condition of distress which can be met by the action of specific inhibitors. The model of fundamental distress and its inhibition which is proposed here may serve to provide a testable alternative to the current metaphysics of anxiety (May, Angel, & Ellenberger, 1958).

#### SUMMARY

Contemporary theories of anxiety, while showing divergency in the statement of specific antecedents for and indicators of anxiety, have shared a dual emphasis. They have called on an archetypical evoker of anxiety (or trauma) to explain the first occurrences of anxiety and on the association of neutral events with the archetypical evoker in order to account for learned, symptomatic, or secondary anxiety. The second communality has been an emphasis on flight (escape or avoidance) from trauma or its signals as the basic mechanism for the control of anxiety.

\* It is tempting to speculate that tendencies in the older organism to be active (Bühler's *Funktionslust*, 1930, or White's "competence motivation," 1959) may be related to the repeated arousal of distress as a consequence of the withdrawal or omission of a well-entrenched inhibitor of anxiety. In other words, the interruption of well-established behavior sequences may lead to anxiety and their continuation may ward it off. Such a position would suggest that in psychotherapy it may be as profitable for the patient to be able to complete interrupted behavior sequences as it is for him to avoid traumatic events.

We have presented evidence to suggest that the "flight from trauma" view of anxiety is incomplete. Specifically, the conception of pain as the sole source of later anxiety has been shown to be untenable.

Two supplements to the traumatic theory of anxiety were proposed. The burden of one was to point out the occurrence of anxiety or distress in the absence of any clearly discernible antecedent trauma. The periodic distress of the human newborn was taken as an example of this phenomenon. The second modification suggested that anxiety may be controlled not only by flight from trauma and its signals but may be reduced by the action of specific inhibitors. These inhibitors may be responses of the organism (for example, the sucking response of the newborn) or external environmental events (for example, Harlow's experimental "mother").

The implications of these supplementary proposals for a theory of anxiety were explored.

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## A REACTION POTENTIAL CEILING AND RESPONSE DECREMENTS IN COMPLEX SITUATIONS<sup>1</sup>

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On the basis of Hullian drive theory, it is not difficult to account for the relatively poorer performance of high drive subjects when the correct response is nondominant, or the superior performance of high drive subjects in some instances when the correct response is dominant. The relationship  $E_1 - E_2 = i[D(H_1 - H_2)]$  yields the prediction that increased drive ( $D$ ), by multiplying differences in habit strength ( $H$ ), leads to an increase in the difference between the reaction potential ( $E$ ) of the primary response and the reaction potentials of competing responses.<sup>2</sup> When the dominant tendency is correct, higher drive will favor the occurrence of correct responses, and when the dominant tendency is incorrect, higher drive will detract from performance because the probability that oscillation will bring the  $E$  of the dominant incorrect response below that of the correct response is decreased.

A more difficult problem is posed by the occurrence of decrements in the performance of a dominant response related to increased drive. Drive related response decrements have been noted in two types of situations: with very strong drive levels, and also with high habit strength and less extreme drive levels. When drive is varied, the relationship between drive level and the performance of a dominant response seems to take the form of an

inverted **U**, with more moderate drive levels being optimal for performance. This relationship has been noted in a number of reviews of drive studies (Cofer, 1959; Courts, 1942; Duffy, 1957; Hebb, 1955; Kendler, 1959; Malmö, 1958, 1959). In these reviews drive is often defined physiologically or in terms of muscular tension level. Data suggesting that such drive variables have a functional correspondence to the more usual Hullian definitions of drive are presented by Lovaas (1960b) and by Malmö (1959). As would be expected from such correspondence, a number of experiments which do use the more usual Hullian drive producing operations such as water deprivation (Kendler, 1945; Malmö, 1959), food deprivation (Thomas & King, 1959), and strength of aversive stimulation (Brush, 1957; Kaplan, 1952), have also shown the inverted **U** shaped relationship between drive and performance. When performance is studied at two drive levels over varied levels of habit strength, the results of several recent studies (Ramond, 1953; Spence, Farber, & McFann, 1956; Spence, Taylor, & Ketchel, 1956) indicate that high drive which facilitated the performance of a dominant response at low  $H$  levels, led to performance decrement at high  $H$  levels. In these studies, the performances of high and low anxiety groups were compared in verbal learning tasks where the initially dominant response was correct. In each of these studies the higher anxiety (drive) group was initially superior, but became inferior

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<sup>2</sup> In the body of the text  $E$  is used for  ${}_aE_n$  and  $H$  for  ${}_aH_n$ .

in later learning trials. These results have been replicated by Lovaas (1960b) using levels of dynamometer tension to induce drive differences. A study by Malmo and Amsel (1948) also shows a decrement at the end of training, when high and low anxiety groups are compared.

In the Hullian system, increased drive multiplies habit strength differences between dominant and competing responses. Therefore, higher drive should be facilitating throughout the range of drive and should be especially facilitating later in learning as the habit strength of a dominant response increases relative to the habit strength of competing responses. Therefore, evidence that higher drive can facilitate performance at lower levels of drive and lower habit strength of the dominant response, but leads to response decrement at high *D* and *H* levels would not seem to be explicable in terms of the basic Hullian position. A number of accounts which use elaborations of the basic Hullian position have been offered to explain some of these occurrences of response decrement. These accounts have utilized hypotheses concerning drive stimuli, inanition, and threshold levels. However, the evidence now available, and the criticisms of these accounts raised by Hill (1957) and by Malmo (1959), indicate that these accounts may also be incomplete. We will briefly review some of the findings which do not seem to be adequately explained by these accounts, and will suggest an alternate explanation of drive related decrements based on a reconsideration of Hull's reaction potential ceiling.

#### CURRENT ACCOUNTS OF DRIVE RELATED DECREMENTS

Both Munn (1951), in discussing the response decrements obtained by

Warden (1931) and by Hull (1952) in his drive postulate, attribute response decrements after protracted deprivation to physical weakening. However, as Malmo (1959) points out, drive related response decrements are reported in studies where either the drive conditions used (Kaplan, 1952; Stennett, 1957), physiological measures (Malmo, 1959), or continuing increases in activity level (Thomas & King, 1959) tend to rule out organism weakness as a factor in producing the decrements. In the Kaplan study the drive condition was a bright light and the dependent variable was rate of pressing a Skinner bar to escape the light. The drive was thus an aversive condition external to the organism and so should not produce inanition. As would be expected, there was an increasing rate of responding to a point between 111 and 530 millilamberts, but higher light intensities produced a decrease in rate of response. These data are partially confirmed by a study by Keller (1941), although in Keller's study, light intensity was not varied much beyond the point of response rate decrement. Stennett's (1957) use of varying instructions as drive conditions with counterbalanced order of presentation would also seem to make inanition explanations of the response decrements in this study less likely. The data reported by Malmo (1959) are from a study by Bélanger and Feldman on the relation between Skinner box responses and water deprivation. In this study, increase in heart rate throughout the range of drive provides indirect evidence against organism weakening. More direct evidence is provided by Thomas and King (1959). As drive was increased in a generalization situation, responding to the training stimulus increased to a point, then decreased, while the total

frequency of the same response to all stimuli continued to increase.

Drive stimulus ( $S_D$ ) accounts of drive related performance decrements have also been widely used.  $S_D$  accounts which are consistent with Hullian theory have been of two types: one emphasizing competing responses conditioned to the stimuli of high drive (e.g., Child, 1954; Rodnick & Garnezy, 1957), the other explaining response decrement under increased drive as a function of drive stimulus generalization gradients (Yamaguchi, 1952).

The competing response account usually assumes that as a result of an organism's past experience, task irrelevant or competing responses have become attached to the cues of high drive. The energizing effects of moderate drive increases may facilitate relevant performance until drive reaches the levels which increasingly cue in the competing responses, thus producing decrements in performance. In such a case, the relationship between drive and performance can take the form of an inverted **U**. This hypothesis seems especially applicable when the drive is anxiety which elicits avoidance or withdrawal which interferes with the efficient functioning of the organism. However, there is evidence that the inverted **U** relationship between drive and performance is found when there has been no previous experience with the stronger drive conditions which are associated with response decrement, and therefore, previous conditioning of competing responses to the highest drive cannot account for the observed response decrement (Denenberg & Bell, 1960; Thomas & King, 1959; Warden, 1931). It can still be assumed that innate avoidance responses are cued when any drive becomes extreme. However, there is evi-

dence that avoidance responses are facilitated under moderate aversive drive and decreased under extreme aversive drive (Brush, 1957; Denenberg & Bell, 1960; Kaplan, 1952). This decrease in avoidance responses under high drive suggests that innate avoidance responses are not responsible for high drive response decrements. It is still possible, however, to hypothesize that the particular avoidance responses used as dependent variables in these studies were different from, and competed with other innate avoidance responses.

The  $S_D$  generalization account of response decrement can explain many performance decrements associated with changed drive as being a function of lowered generalized habit strength because of different drive levels providing different stimuli than those present during training. However, in a number of studies where drive related response decrements have been observed,  $S_D$  accounts are not applicable (Lovaas, 1960b; Malmö & Amsel, 1948; Ramond, 1953; Spence, Farber, & McFann, 1956; Spence, Taylor, & Ketchel, 1956). In these studies a between-groups design was used with respect to the drive variable and high drive was related to response facilitation at low habit strength and response decrement at high habit strength. Because drive did not change within subject groups, the response decrement in the high drive group only at higher  $H$  levels cannot be explained in terms of changes in drive stimuli. In the Thomas and King (1959) study  $S_D$  changes also did not seem to contribute to the observed high drive response decrement. Control data for groups trained under different drive levels indicate that frequency of responding to a stimulus was a function of drive level at testing rather than change in drive from testing to training.



Dominant response decrements under high drive conditions have also been explained in terms of increased numbers of competing responses raised above threshold by the increased drive (Farber & Spence, 1953; Mednick, 1958; Spence, 1956, 1958; Taylor, 1956; Taylor & Spence, 1952). This explanation can be illustrated by a situation where the  $E$  distribution of a dominant response is partially above threshold, and yet overlaps the distributions of competing responses which are entirely below threshold. In such a situation, no competing responses can occur, and the primary response will eventually occur. Increased drive, acting in such a situation can raise the  $E$  distributions of competing responses partially above threshold, enabling those responses to occur, where only the dominant response could occur previously.

This threshold account seems very adequate in explaining a drive induced decrease in the proportion of dominant to competing responses when habit strength is low enough or the level of the high drive condition is low enough for the  $E$  distribution of the dominant response to be partially subthreshold. When habit strength or drive is sufficient for the  $E_1$  distribution to be completely above threshold, there would be no threshold effect and a higher drive condition would then be expected to facilitate  $R_1$  (when  $H_1 \neq H_2$  - Spence, 1958, p. 98.). However, the reviews and studies cited earlier all suggest that  $R_1$  decrements are also found at extreme drive levels or at high levels of habit strength.

Consideration of the type of dependent variable used in studies showing high drive response decrements suggests a possible additional limit to the generality of the threshold account of such decrements. The threshold account would not seem applicable to

studies where the dependent variable is the frequency of occurrence of the correct response within a limited time interval. While within the Hullian system exact prediction of response rate is quite complex and must involve a number of assumptions about number and relative strength of competing responses and distance of dominant and competing responses above or below threshold, some general statements concerning the predicted interaction of drive and response rate can quite easily be made. The threshold account predicts the largest decrement in a dominant response under high drive when under low drive there are a number of competing responses which are minimally subthreshold and where the dominant and competing responses have the most overlap (Taylor, 1956). Accordingly we will assume a situation with a large number of competing responses ( $R_e$ ), all with their maximum  $E$  oscillation values only slightly subthreshold, and a dominant response ( $R_1$ ) with 1% of its  $E$  distribution above threshold.  $R_1$  is then the only response that can occur and its probability of occurrence varies from .01 during a time interval just long enough for the response to occur to 1.0 over unlimited time intervals. In any limited time interval the probability of  $R_1$  is  $< 1.0$ . Higher drive, which moves a portion of the distributions of the competing responses suprathreshold, increases the probability that one of the many  $R_e$  will occur and thus increases the total number of responses, but also maintains or increases the  $E_1 - E_e$  distance. ( $E_1 - E_e$  stays the same when there have been equal numbers of the dominant and each of the primary competing responses in Spence's contiguity theory—1958, p. 90. If there have been more dominant responses, or under reinforcement theory,  $E_1 - E_e$  would increase.) The



percentage of the  $E_1$  distribution which is greater than the maximum  $E_c$  under high drive is thus equal to or greater than the percentage  $E_1$  which was suprathreshold under low drive. Because at the high  $E_1$  the latency of the dominant response is less, the frequency of  $R_1$  should then be at least as great under high drive even though the frequency of competing responses has increased. The reduction is in response nonoccurrence. Therefore, we would interpret Hullian theory, as it now stands, as predicting that, regardless of the number of competing responses, degree of overlapping between dominant and competing responses, and percentage of the dominant response distribution above threshold, *during any limited time interval* the frequency of occurrence of  $R_1$  would be at least as great under high drive as under low drive. Therefore, response decrements under high drive as compared to low drive in studies involving frequency of  $R_1$  in limited time intervals or  $R_1$  latency as the dependent variable (e.g., Brush, 1957; Kaplan, 1952; Thomas & King, 1959) suggest that additional explanatory principles may be needed.

There is a great deal of empirical evidence on the interaction of drive, habit strength, and response choice which is consistent with current Hullian theory (cf. Spence, 1956, 1958; Taylor, 1956). However, the evidence reviewed above suggests that current Hullian theory may present an incomplete account of certain situations under which response decrements have been noted.

#### REACTION POTENTIAL CEILING HYPOTHESIS

Optimal levels for both drive and habit strength suggest that consideration of maximum level of reaction potential, which is a joint function of  $D$

and  $H$ , may aid in providing an account of drive related response decrements. Hull (1943) initially set a maximum  $E$  equal to the product of  $D$  ceiling and  $H$  ceiling. Spence (1958), in discussing studies in which weak shock led to diverging learning curves and strong shock to converging learning curves, with speed of response as the dependent variable, suggests that "It is possible that one runs into a ceiling effect or physiological limit in the case of this behavior measure" (p. 77). The wide range of dependent variables in studies where nonmonotonic relations between  $D$  and  $H$  and performance are found suggests the utility of hypothesizing a reaction potential ceiling lower than that indicated by Hull and not restricted to specific behavioral measures. This hypothesis is consistent with the rest of the Hullian system and would extend the explanatory power of Hullian theory. Figure 1 illustrates what would be expected on the basis of the  $E$  ceiling hypothesis regarding the interaction of drive level and the reaction potentials of a dominant response and a weaker, competing response tendency. Figure 2 represents the predictions made regarding response probability or frequency for the two responses in this simplified situation. To simplify computations (and following Spence's current position) oscillation distributions are assumed to be normal (Spence, 1956). As is shown in Figure 2 the  $E$  ceiling can account for nonmonotonic or inverted U relationships between drive and the performance of a dominant response if at least one competing response tendency is present. Up to Point A, drive increases lead to increases in the difference between  $E_1$  and  $E_2$  and therefore to an increase in the probability of  $R_1$ . When  $E_1$  reaches its ceiling, further increases in  $D$  serve to increase only  $E_2$ , so that  $E_2$  begins

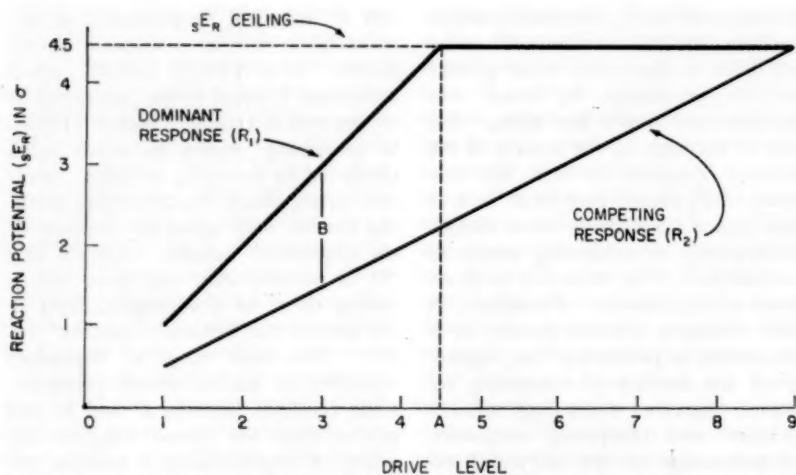


FIG. 1. Effects of drive level on reaction potentials ( $sE_R$ ) for a dominant response ( $R_1$ ) and a competing response ( $R_2$ ) with an hypothesized  $sE_R$  ceiling.

to overtake  $E_1$  and momentary  $E_1$  more frequently oscillates below  $E_2$ , increasing the frequency of occurrence of  $R_2$  and reducing the frequency of  $R_1$ .

The reaction potential ceiling hypothesis also provides an account of ini-

tial superiority and later inferiority of high drive subjects such as was found in the Spence, Taylor, and Ketchel (1956) verbal learning study and thus would meet Hill's (1957) criticisms of Hullian theory. In a learning situa-

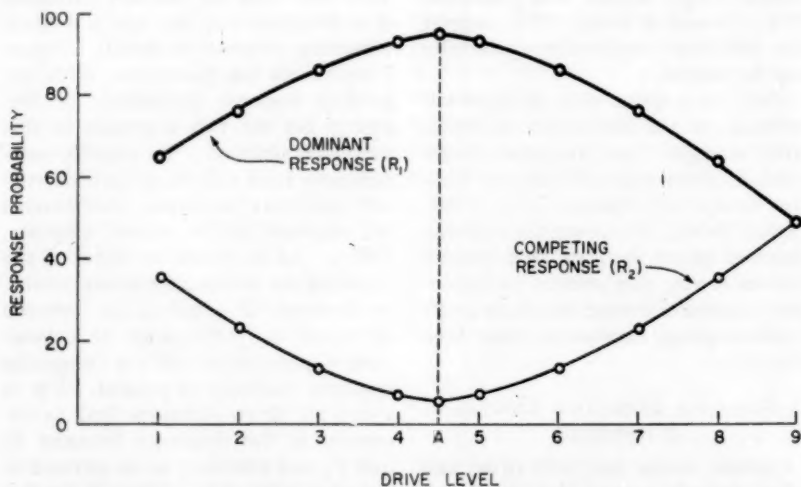


FIG. 2. Expected probabilities of a dominant response ( $R_1$ ) and a competing response ( $R_2$ ) computed as a function of drive level on the basis of the  $sE_R$  ceiling hypothesis. (Point A corresponds to Point A in Figure 1.)

TABLE 1  
HIGH AND LOW ANXIOUS SUBJECTS AT  
BEGINNING AND LATER STAGES OF A  
HYPOTHETICAL LEARNING TASK  
( $sE_R$  ceiling = 6)

Stage	$sH_R$	$sE_R$	
		Low $D$ Subjects ( $D = 5$ )	High $D$ Subjects ( $D = 10$ )
Beginning			
Correct $R$	.5	2.5	5.0
Incorrect $R$	.4	2.0	4.0
$sE_R$ difference		.5	1.0
Later			
Correct $R$	1.0	5.0	6.0
Incorrect $R$	.4	2.0	4.0
$sE_R$ difference		3.0	2.0

tion which is sufficiently competition but with the correct response initially the most probable response, high drive subjects would be expected to be superior in early trials. Habit strength would increase at the same rate in both high and low drive subjects, but  $E_1$  would reach its ceiling earlier for the high drive group while  $E_s$  of competing responses remain at higher levels than for the low drive group. In later stages of learning, the  $E_1$  for the low drive group can approximate the  $E_1$  for the high drive group, while the strengths of competing responses are lower for low drive subjects. We are assuming the ceiling for high and low drive subjects is the same. Illustrative calculations representing predictions from the  $E$  ceiling hypothesis for a hypothetical learning situation are presented in Table 1. Extinction of competing responses is not taken into account in these calculations but would only enhance the cross-over effect, since competing responses would extinguish more rapidly for the low drive group because of their greater frequency for

this group early in learning. Such cross-overs would be expected to occur only in learning situations that are sufficiently competition. Studies employing less competitive tasks with moderate drive levels (high anxious normal subjects), such as Experiment I in Spence, Farber, and McFann (1956) and experiments on drive variation in classical conditioning would not be expected to show this cross-over, because  $H_2$  would not be large enough so that high drive could cause  $E_1$  and  $E_2$  distributions to overlap.

Actual response decrements with increasing drive or habit strength can, therefore, be accounted for on the basis of the  $E$  ceiling only if there are competing responses which interfere with performance after the  $E$  ceiling is reached, or possibly on the basis of a combination of the  $E$  ceiling and inhibition acquired through continued responding (Prokasy, 1960; the evidence discussed suggests this phenomenon is not too powerful). If inverted U shaped relationships occur between drive level and response measures in noncompetition situations, especially with relatively distributed trials or responses requiring small effort, additional hypotheses will be required. Of course, the criterion for a noncompetition situation should not be whether or not only one response is recorded. There may be competing but unrecorded responses. For example, when drive is varied to include sufficiently high levels, the U shaped relationship between drive and performance would be expected with operant bar pressing by human subjects (King, Merrell, Lovinger, & Denny, 1957) or the pecking response in pigeons (Thomas & King, 1959) even though only one response is recorded in such studies. Perhaps peripheral (noncortical) physiological measures are least likely to be influ-

enced by response competition. The inverted  $U$  was not discovered in investigations of the effects of antecedent drive conditions on knee jerk amplitude (Courts, 1939), heart rate (Malmo, 1959), palmar conductance and electromyogram recordings (Stennett, 1957). There are some indications that the inverted  $U$  may hold for the relationships between hunger drive and salivation (Finch, 1938), and between muscular tension and eyeblink rate (Lovaas, 1960a), although both responses are notoriously susceptible to interference from other activities of the organism.

Because the  $E$  ceiling hypothesis implies that decrements in a dominant response can be expected at high levels of  $H$  and  $D$  in any situation with sufficient response competition, the response relationships found in the studies that have been cited should be replicable in a variety of experimental situations. For example, animals at 90% ad libitum body weight in a  $T$  maze with forced trials used to train  $H_1$  to 1.0 and  $H_2$  to .7, should show an inverted  $U$  shaped relationship between drive and percentage  $R_1$  when tested under different fractions of ad libitum weight. Running speed measures should continue to increase past the point of maximum  $R_1$ . Also, under the  $E$  ceiling hypothesis, the point of maximum  $R_1$  should be at higher drive levels if  $H_1$  is less than 1.0. This is because with lower  $H_1$  a higher drive level would be required to bring  $E_1$  to ceiling levels. The inverse relation between  $H_1$  and drive level required for maximum  $R_1$  should also hold for different methods of varying  $H_1$ . An example would be a generalization situation where, with sufficient response competition, this inverse relationship would imply a flattening of generalization gradients given drive levels leading to decrement at the train-

ing stimulus, but facilitating  $R_1$  at the generalized stimuli where  $H_1$  is less.

#### CLINICAL IMPLICATIONS

The behavior of some psychiatric patients seems to be characterized by an increased probability of responses usually of low rank within normal response hierarchies, and decreased probability of responses which are usually dominant. Because the  $E$  ceiling hypothesis predicts this kind of response change under strong drive conditions, it may be useful in generating descriptions of clinical phenomena.

Under the  $E$  ceiling hypothesis moderate anxiety (drive) would usually be expected to facilitate performance and extreme anxiety would lead to many of the competing  $R$ s in a response hierarchy achieving approximately equal reaction potentials (at or near ceiling strength). Some of the responses usually lower in response hierarchies to a stimulus are generalized from other stimuli. High drive pushing many responses to ceiling or near ceiling levels would tend to raise the probability of the more remote, generalized responses which had been of lower rank under normal drive conditions. An individual under extreme anxiety would thus be expected to respond with a mixture of appropriate (initially dominant through social reinforcement), slightly less probable, and remote associates. Both motor sequences and concept association chains would be disturbed because of the intrusion of competing responses usually of low probability. This seemingly randomized, fragmented behavior expected under extreme drive is very close to that found in acute schizophrenia. For example, Bleuler (1950) lists disorder of the process of association as the most important characteristic of schizophrenia and says:

Especially in acute conditions of schizophrenia, one often finds so complete a fragmentation of the thinking process that they cannot result in a complete idea or action . . . (p. 350).

Gottesman and Chapman (1960), describing the kinds of reasoning errors made by schizophrenic research subjects, note that the "error can be described as a more random response by schizophrenics" (p. 253) rather than arising from a more specific kind of paleologic thinking (Arieti, 1955).

In his account of schizophrenia, Mednick (1958) shows in much greater detail the close correspondence between schizophrenic behavior and the behavior expected when high drive increases the probability of responses initially lower in response hierarchies. A possible difficulty in the Mednick account is that the threshold argument is used to explain the increased competition from remote responses (see the above discussion of the threshold argument, and the criticisms of this account by Child, 1954, and Hill, 1957).

The association between extreme anxiety and fragmented, disorganized behavior is not restricted to patients diagnosed as schizophrenic, but seems to occur whenever anxiety is sufficiently severe regardless of diagnosis (Alexander, 1948; Coleman, 1956; Hutt & Gibby, 1957; White, 1956). From a review of the literature on anxiety, White concludes that while moderate degrees of anxiety seem to facilitate organized behavior, severe anxiety is related to a breakdown of ordered behavior. Under extreme anxiety:

Thought and judgment deteriorate, actions are erratic and poorly controlled, new acts are started before old ones are completed (White, 1956, p. 208).

Grinker and Spiegel (1945) state that these fluctuating behavior patterns also

occur in combat anxiety cases, and note the similarity of symptoms in cases of severe anxiety and acute psychosis. However, moderate combat anxiety is reported as having a facilitating effect on performance (Wickert, 1947).

When disordered responses and high drive are initially present, the development of the more rigid, simplified symptom structure of the chronic patient would be explained in terms of differential reinforcement of certain responses through association with anxiety reduction (Mednick, 1958). On the other hand, when stress reduction is sharp and does not continue to be functionally related to symptom production the recovery rates would be expected to be high.

In reviewing severe combat anxiety syndromes, Cattell (1950) notes that there is "little or no association with earlier neurotic symptoms" and that "they are direct responses to the stress of environment and show high recovery rates (95%) when the stress ends" (p. 502). The clinical phenomena associated with moderate and extreme anxiety thus seem to be consistent with the types of response patterns that would be associated with moderate and strong drive under the *E* ceiling account.

#### SUMMARY

In Hullian learning theory, increased drive should facilitate the performance of the strongest response in a response hierarchy. However, there is considerable experimental evidence that, while moderate drive increases tend to facilitate performance, high drive is related to decrease performance of a previously dominant response. It was suggested that these findings could be handled within the Hullian framework by the use of a lower reaction potential ceiling than that originally suggested by Hull. The additional utility of the

reaction potential ceiling in accounting for some clinical phenomena was then discussed.

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## THEORETICAL NOTES

### A CRITICAL NOTE ON THOMPSON'S TWO-FACTOR THEORY OF INHIBITION

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Motivated by inadequacies in the two major theories of spontaneous alternation behavior—reactive inhibition ( $I_R$ ) theory and Glanzer's (1953b) stimulus satiation ( $I_S$ ) theory—Thompson (1960b) has recently developed a "two-factor" theory which combines the  $I_R$  and  $I_S$  concepts in conjunction with some novel assumptions. In essence, Thompson assumes that only  $I_R$  is developed in rewarded subjects while both  $I_R$  and  $I_S$  are assumed to occur in nonrewarded animals. The development and dissipation of  $I_R$  follow a simple negative growth function of time;  $I_S$  dissipates very slowly over time and may be generalized to similar stimuli. The theory is applied to the alternation situation and to the phenomena of extinction, reminiscence, and spontaneous recovery. The present note bears only on the theory as it applies to alternation. Its purpose is to point out some features of Thompson's (1960b) system which are highly questionable in light of existing data.

1. According to *Deduction 1*, "Nonrewarded Ss will alternate more than rewarded Ss." This seems a very reasonable prediction to make, and Thompson reports verification of it in his own research. Walker (1956), however, using water as reinforcement, reports the opposite result. Even interpreted conservatively, Walker's data certainly do not show rewarded subjects to alternate less frequently than nonrewarded subjects.

In another of Walker's (1958) experiments, rats reinforced with shock reduction, and tested with a short intertrial interval, showed about 93% alternation. While there is no nonrewarded group in that study with which to compare the

rewarded subjects, it is unlikely that such a group would have alternated at a higher level than 93%.

In this same regard, a considerable amount of alternation (about 86%) was also found by Fowler, Blond, and Dember (1959) to prevail in the first 4 days of testing in rats run under various conditions of food reinforcement.

It should be noted that there are data that reveal decreased alternation with increasing reinforcement (e.g., Fowler, Fowler, & Dember, 1959); however, there are enough instances of high percentages of alternation after reinforced trials to make questionable the universal validity of Thompson's *Deduction 1*.

2. *Deduction 3*<sup>1</sup> states that "Rewarded Ss will alternate turns and repeat stimuli." Thompson mentions that in his own experiment (1960a) "this deduction has not been verified." Contradictory results are also furnished by Montgomery (1952), who obtained stimulus alternation in a + maze under reinforcement conditions. Also relevant here are studies by Rothkopf and Zeaman (1952) and Estes and Schoeffler (1955), in both of which stimulus alternation was obtained with reinforced subjects.

3. *Deduction 4* states that "Nonrewarded Ss will alternate stimuli and repeat turns when the mazes are dissimilar." The results of at least three experiments (Dember, 1958; Glanzer, 1953a; Walker, Dember, Earl, & Karoly, 1955) support this deduction.

However, in developing the argument on which the deduction is based, Thomp-

<sup>1</sup> Deductions 3-6 refer to the + maze situation, where the subject is run from opposite starting boxes.

son indicates that the deduction will hold only after "a few trials," over which  $I_S$  can develop and accumulate and overcome the opposing effect of  $I_R$ , and between which  $I_R$  can dissipate. Though there seem to be no relevant published data from + maze studies, it is our strong impression that stimulus alternation will occur after a single trial. This certainly is true of the response-to-change phenomenon (e.g., Dember, 1956; Walk, 1960), which presumably would be explained by Thompson via the stimulus satiation concept. This issue, however, is not a major one, since the same deduction can be made on the more likely assumption that the  $I_S$  from a single exposure is of greater magnitude than the  $I_R$  from a single response.

4. *Deduction 5* asserts that "Nonrewarded Ss will alternate turns and repeat stimuli when the arms are similar." Thompson reports verification of this deduction in his own experiment. In a + maze having two black goal arms there was only 35% stimulus alternation. This compares with the 60% stimulus alternation obtained in a + maze having one black and one white goal arm.

While the above results are in conformity with the deduction, there is a set of data that can be interpreted as contrary to it. Arguing from much the same basis as Thompson, Dember (1958) predicted that blind rats would show more response alternation than normals in a black-white + maze. For the blind rats, the two goal arms should be more "similar" than for the normals; yet both groups showed the same, above-chance percentage of stimulus alternation. However, it is possible that loss of vision sensitized the animals to other stimulus dimensions (auditory, olfactory) and thus made the arms dissimilar on this basis.

5. *Deduction 6* and its attendant discussion read as follows:

Rewarded Ss will show a decrease in turn alternation and an increase in alternation of stimuli with an increase in intertrial interval.

The stimulus satiation theory makes the opposite prediction, i.e., a decrease in alternation of stimuli with an increase in intertrial interval, since Glanzer assumes that

stimulus satiation effects dissipate with time. The two-factor theory, assuming that only  $I_R$  develops when the responses are reinforced, would predict a decrease in turn alternation as the  $I_R$  dissipates. Since the S can alternate turns or stimuli but not both, any decrease in turn alternation must lead to an increase in the alternation of stimuli. Verification of this deduction was found in Thompson's . . . study; the percentage of turn alternation for 30-sec., 30-min., and 24-hr. distribution was 40, 29, and 28, respectively (p. 203).

This deduction is quite puzzling. If we understand the situation correctly, the two-factor theory states that in rewarded subjects the sole basis for alternation is the development of  $I_R$  (Deduction 3), generated by such events as turning responses in the maze (see Footnote 1). The growth and dissipation of  $I_R$  follow a simple negative growth function of time (see Thompson's Statement 1, 1960b, p. 201). According to Glanzer, the sole source of alternation for both rewarded and nonrewarded subjects is  $I_S$ , which is generated by exposure to stimuli, and which grows and decays as a simple negative growth function of time. Glanzer predicts that in a T maze with dissimilar arms all rats will exhibit alternation behavior that is significantly above chance (50%) for short intertrial intervals, with a reduction to chance level as intertrial interval increases; i.e., the subjects will stop alternating. In any T maze, independent of stimulus conditions, Thompson should predict that rewarded rats will manifest a significant percentage of alternation that will decline to chance level (50%) as the intertrial interval increases. Thus, in a T maze situation both theorists predict identical behavior, although they explain it in different ways.

In a + maze, however, the rat is forced to choose between stimulus alternation and response alternation, since one is the converse of the other. Glanzer predicts significant stimulus alternation in the + maze, which will drop to chance level as the intertrial interval increases. Thompson predicts significant response alternation in the + maze, which will drop to chance level as the intertrial

interval increases. The fact that the decline in stimulus alternation forces an increase in response alternation, or that a decline in response alternation forces an increase in stimulus alternation, has little meaning, for these increases are artifacts of the + maze situation. In particular, there is nothing in his theory that should lead Thompson to expect the apparent increase in stimulus alternation, with increasing intertrial intervals, to rise significantly above the 50% level. Moreover, with regard to Thompson's data, the low response alternation percentages suggest that the subjects were exhibiting above-chance stimulus alternation, in contradiction to the two-factor theory.

Ignoring its bearing on Thompson's theory, one might consider the 12% increase in stimulus alternation detrimental to Glanzer's theory. But even this would be a questionable assertion for at least two reasons. The increase may well be insignificant. In addition, Thompson's procedure for measuring alternation over 30-minute and 24-hour intervals involved a comparison of Trial 2 behavior on one block with Trial 1 behavior on the subsequent block. For half the blocks, the subject started from the same stem (T maze), while for the other half the subject started from opposite stems (+ maze). The sequence of blocks was randomized for each subject, thereby confounding the 30-minute and 24-hour interval data with differences in maze type.

Empirical opposition to Deduction 6 is also found in studies (Rothkopf & Zeaman, 1952; Walker, 1956; Zeaman & House, 1951) which found alternation in a T maze after a single rewarded trial to occur over intertrial intervals of 30 minutes or more—a duration which seems rather long for the dissipation rate postulated for  $I_R$  in Thompson's theory.

6. *Statement 2 reads:* "Whenever an organism is exposed to a stimulus in the absence of reward, only  $I_S$  will develop." Thompson does not explicitly make the following deduction, but it follows directly from Statement 2 and from the assumption that  $I_S$  develops only in non-

rewarded subjects: If the subject is placed in a goal box, no  $I_R$  will develop. Furthermore, no  $I_S$  will develop, according to the theory, if the subject is rewarded. Thus, in the special situation in which the subject is both placed in a goal box and rewarded, then neither  $I_R$  nor  $I_S$  will develop, and there should be no alternation exhibited. This deduction is contradicted in an experiment by Sutherland (1957).

In general, the major criticism of the two-factor theory is that much of the empirical data on alternation does not agree with some of the postulates. While it may be argued that these data are contaminated or are, in some way, inappropriate for testing the theory, there seems to be little rational basis for this objection. All of the studies we have cited appear to meet the specifications or requirements set by the formal statement of the theory.

The chief problem arises from the assumption that limits the operation of the stimulus factor,  $I_S$ , to nonrewarded subjects. This leaves the response inhibition concept,  $I_R$ , to account for the alternation of rewarded subjects. Unfortunately, the evidence indicates that  $I_R$  does not provide an adequate explanation. It is this inadequacy which has led to alternative formulations (Berlyne, 1960; Glanzer, 1953b; Walker, 1958). More precisely, two facts have emerged from the recent work on alternation, both of which, in effect, indicate that  $I_R$  is not an important factor.

First, the major sources of alternation are environmental stimuli. Reactive inhibition, at least when it is defined as in Thompson's usage (as generated by a specific response, such as turning right or left), does not seem to influence alternation under the usual experimental conditions involving standard T and + mazes and normal rats. It does seem that  $I_R$  can be made more effective if operations are undertaken to make the turn response more distinctive or important. Walker, Dember, Earl, Fawl, and Karoly (1955) obtained a small but significant amount of response alternation by making the turn a twisting, climbing

one. In general, however, the data suggest the presence of a hierarchy of alternation sources, with the response component ( $I_R$ ) serving as a rather low priority member.

Secondly, low to moderate levels of drive or reinforcement *do not* alter alternation behavior as compared with non-reinforcement conditions; i.e., the behavior of rewarded and nonrewarded subjects is the same. As reinforcement is increased to high values, alternation tends to disappear. The fact that alternation does persist to the degree that it does in spite of reinforcement is one of the most interesting aspects of the phenomenon. A complete understanding of the relationship between reinforcement and alternation is not yet available, but there are no data which suggest that  $I_R$  becomes a more potent source of alternation under reinforcement conditions.

With regard to the behavior of non-rewarded subjects, Thompson's inclusion of the stimulus satiation concept leads to predictions which are more congruent with existing data, and which are generally similar to those of Glanzer's theory. The only apparent difference between the  $I_S$  concepts of the respective theories is that Glanzer postulates a rapid dissipation of satiation, while the two-factor theory postulates a very slow decay. The question of duration of effects is complex, but the lengths of intervals over which alternation has been obtained seem inconsistent with the dissipation rate postulated by Glanzer and more nearly consistent with Thompson's assumption.

In conclusion, all of the above suggests that the problem of alternation has not been settled by the two-factor theory, at least in its present form.

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## WORD VALUES, WORD FREQUENCY, AND VISUAL DURATION THRESHOLDS:

### A COMMENT

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Johnson, Thomson, and Frincke (1960) identify word value and word frequency as two of the critical determinants of visual duration thresholds, and then attempt to elucidate the interrelationships of these concepts. Their definition of word frequency follows that of Solomon and Howes (1951) in accepting the Thorndike-Lorge tables as representative of the frequency of words in the English language. However, they equate word value throughout their study with the rating of the word on the good-bad scale of the semantic differential. It is with respect to this crucial methodological choice that issue must be taken. Word *value* has been used in the literature to represent a dimension different from that tapped by the *evaluative* scale of the semantic differential, and while the two may share some commonality, it does not seem entirely appropriate to use the latter in order to operationally define the former. This distinction may be further clarified by reference to some pertinent studies.

In an investigation of word value, Postman, Bruner, and McGinnies (1948) demonstrated that words falling within a preferred value area tended to have lower tachistoscopic recognition thresholds than words from other, nonpreferred areas. The rank order of Allport-Vernon Study of Values scores was taken as an operational definition of the hierarchy of value areas for each individual. Leav-

ing aside the question of perceptual selectivity and defense, the word value was seen as being representative of a general value orientation of the individual, and, as such, was related to his underlying motivational system. Solomon and Howes (1951) criticized this procedure on the basis that word frequency had not been considered, but accepted the definition of word value. They state:

we shall use the word *interest* to designate the six fields of orientation; the word *value* will be reserved for the relative degree of orientation of a subject toward any one of these six areas of interest as indicated by his score on the Allport-Vernon questionnaire (p. 257).

Again, what is under consideration is a motivational rather than more superficial attitudinal concept. Postman and Schneider (1951) reiterate that individual differences among subjects in visual recognition thresholds

can be systematically related to "directive" factors in the subjects—their drive states, lasting predispositions and momentary sets, past experiences, and expectations (p. 271).

Later they state:

To some extent at least, the thresholds reflect differential perceptual sensitivity rooted in *lasting predispositions* [italics added] of the organism (p. 277).

It seems apparent then that word value, as used in the literature, refers to an orientation, predisposition, or other form of motivational state. Johnson et al. use the good-bad scale of the semantic differential, which falls along the evaluative dimension, to operationally define word value. However, Osgood, Suci, and Tannenbaum (1957)

<sup>1</sup> This article was written during the tenure of the author as a Postdoctoral Fellow of the National Institute of Mental Health, United States Public Health Service. The author would like to thank Emory L. Cowen for his critical reading of the manuscript.

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define *attitude* toward a concept as the projection of this point onto the evaluative dimension of (semantic) space (p. 190).

Johnson et al. therefore appear to be making the assumption that attitude (which is measured by the good-bad scale) is equivalent to motivation (the implication for behavior of word value). While there is overlap between the concepts, it is quite clear that there is not identity. Thus, their use of "word value" and "word goodness" interchangeably may be called into question, and their conclusions reexamined in that light.

In their Experiment I a series of rank-order correlations ranging from .38 to .63 are found between word frequency and semantic differential goodness. The authors conclude:

Value attributes of words are related to word frequency—and not merely in an idiosyncratic way, but in a far more general sense as well (p. 335).

Clearly the leap from goodness to value is not indicated, and the discovered relationship is between word frequency and word goodness. More important, only word goodness could be investigated in this "general way"; a study of word value would have to take cognizance of the unique individual differences of motivation that are dismissed as "idiosyncratic." In Experiment II subjects were asked to select the "most pleasantly toned" word from a pair of synonyms, one frequent and the other infrequent, and chose the frequent word significantly more often. Again, there is the unwarranted assumption that that which is pleasant will be motivationally important and hence, of value. Further, even to demonstrate the relationship between "pleasantness" and frequency with this design would presuppose that subjects were equally conversant with both words, but preferred the frequent one, and there is no evidence that this is so. In Experiments III and IV a relationship is shown between semantic differential goodness and the frequency of nonsense syllables. In one case the frequency differences are provided by association value and in the

other by experimental manipulation of experience with the syllables. In both cases this provides a replication of Experiment I in eliciting a relationship between word frequency and word goodness, and in neither case is value implied. In fact the use of nonsense syllables emphasizes the fact that these "words" have little if any motivational implication. Finally, in Experiment V, they demonstrated that if words were matched for goodness the more frequent ones would be perceived most quickly, while if they were matched for frequency, the ones with a higher "goodness" rating would be perceived more quickly. This is taken as evidence that value does have an effect on visual threshold. However, in their discussion of the possible underlying factors which produce differential thresholds, Johnson et al. speak of the words as "good," and reduce their "motivational relevance" to differential reinforcing strengths, which appears to be also equated with semantic differential goodness rating. There is no recognition that even though the words "mother" and "vision" are equated for goodness on the basis of semantic differential ratings the two are likely to differ in both motivational relevance and reinforcing strength, and that the extent of this difference will probably vary from subject to subject in an "idiosyncratic way." What Johnson et al. appear to have demonstrated is a relationship between word frequency and word *goodness*; what still remains to be demonstrated is a relationship between word frequency and word *value*.

#### SUMMARY

Johnson, Thomson, and Frincke's interpretation of their work with regard to the interrelationships of word value and word frequency was questioned on the basis of their use of the good-bad scale of the semantic differential as an operational definition of word value. Reference to pertinent previous studies seemed to indicate that word value has a directly motivational connotation while the evaluative dimension of the semantic differential is sensitive to an attitudinal, cognitive component of behavior. An ex-

affirmation of their interpretations of their experimental evidence revealed a failure to take this distinction into account.

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